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THE MATHEMATICAL GAZETTE

EDITED BY
T. A. A. BROADBENT, M.A.

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CONTENTS.

	PAGE
THE IMPORTANCE OF THE HISTORY OF MATHEMATICS IN RELATION TO THE STUDY OF MATHEMATICAL TECHNIQUE. G. J. WHITROW,	225
A SCALE CHANGE CALCULATOR WITH THREE CONTROL POINTS. H. G. GREEN,	228
INITIATIVE IN HIGH SCHOOL MATHEMATICS: INVESTIGATIVE EXERCISES. P. N. ANDERSEN,	230
MATHEMATICS AND CULTURE. J. W. N. SULLIVAN,	243
AVOIDING SYMBOLIC METHODS IN THE SOLUTION OF DIFFERENTIAL EQUATIONS. R. J. A. BARNARD,	254
ON A CONTINUED FRACTION OF STIELTJES. E. G. PHILLIPS,	257
A NEW SEQUENCE IN THE TEACHING OF TRIGONOMETRY. H. ABSON,	259
MATHEMATICAL NOTES (1037-1048). H. BERRY; F. BOWMAN; A. L. DIXON; H. G. FORDER; W. J. H.; E. H. LOCKWOOD; H. V. MALLISON; V. NAYLOR; F. J. D. SYER; D. A. YOUNG; A. L. YOXALL,	261
REVIEWS. W. E. H. BEEWICK; T. M. A. COOPER; D. C. FRASER; N. M. GIBBINS; J. HARGREAVES; D. R. HARTREE; W. HOPE-JONES; E. L. INCE; L. J. MORDELL; M. K. RICE-OXLEY; R. V. SOUTHWELL; G. TEMPLE; C. J. A. TRIMBLE; F. P. WHITE; N. WIENER; J. WISHART,	275
GLEANINGS FAR AND NEAR (869-876),	227
PERSONAL NOTE,	296
INSET,	xiii-xvi

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THE IMPORTANCE OF THE HISTORY OF MATHEMATICS IN RELATION TO THE STUDY OF MATHEMATICAL TECHNIQUE.

BY G. J. WHITROW.

GLAISHER, I believe, once said that no subject loses more than mathematics by any attempt to dissociate it from its history. It is still customary, however, in schools and most universities to regard the history of mathematics as an "extra" outside the ordinary student's syllabus—indeed it is usually treated as an exotic luxury. The point I would like to emphasise is that while the primary aim of the tyro should be the mastery of technique, yet in most cases this mastery is liable to become a mere mechanical acquirement unless it is accompanied by some appreciation of the cultural significance of the "Queen of the Sciences".

A number of excellent treatises, theses and text-books have been written on mathematical history. With very few exceptions these are absolutely distinct from the technical treatises, theses and text-books. To-day we really require a series of books carefully planned to bridge the gap between these two classes—books not designed to usurp the functions of any particular books now in use but to supplement them. Consider the subjects of elementary pure mathematics in the order in which the student is introduced to them: Arithmetic, Pure Geometry, Algebra, Trigonometry, Analytical Geometry and the Calculus. The art of counting goes back to the most primitive civilizations, Pure Geometry to the Greeks and their predecessors, Algebra and Trigonometry to the Alexandrines, Hindus and Arabians, Analytical Geometry to Descartes, and the Calculus to Newton and Leibnitz. In a sense (to a first approximation) there is in this presentation of the elements of mathematical technique a recapitulation of mathematical history analogous to the recapitulation in our embryonic life of our various evolutionary stages. Bearing this analogy

in mind we can visualise the rough framework of one possible pedagogic method of correlating the technical and cultural aspects of our science.

Schopenhauer used to talk with contempt of that which he styled " Professors' philosophy for philosophy professors ", implying that mere uninspired technique results in poverty of conception and aridity of thought. This is true not only in philosophy and in art (look at the Academy !), but also in science generally and in mathematics in particular. Belle-lettrists and other busybodies whose derision of mathematics is merely the ineffectual means whereby they endeavour to conceal their abysmal ignorance are for ever harping on the " inhuman " and " soul-destroying " qualities of the exact sciences. I maintain that by emphasising the historical aspect of these pursuits and their " conscious " and " unconscious " relations with human affairs in general all such qualities, if they exist, may be effectually transmuted. Moreover, even a superficial acquaintance with the cultural development of mathematics is of value in clearing up many apparent obscurities in technique itself. A case in point is Euclid's definition of a straight line, which is to the effect that a straight line is a line which lies equally between its constituent points. [I am aware that Euclid is no longer read in schools, but anyone who aspires to be a mathematician should make himself acquainted with the Elements.] The definition is bad first inasmuch as no use is made of it in the subsequent development of the theory, and secondly because it is obscure. Nevertheless despite the many faults of which we may accuse Euclid no one can honestly assert that he was not aware of what he was talking (*pace* Bertrand Russell's definition). Now Sorel suggests that the clue to the meaning of this definition lies in the art of the mason. " The latter in order to verify the facing of a chiselled surface applies to it a stone-rule coated with red oil. If the facing be perfect the imprint made by the rule appears without any break ; if not there are gaps. Hence the definition." The importance of stone-masonry in Greek civilization may also account for the discovery and investigation of the regular polyhedra at so early a period. I cite these two trivial instances in order to illustrate the light which a little historical knowledge may shed upon certain difficulties and paradoxes in the purely formal development of the mathematical sciences.

As a more profound example consider the problem of the One and the Many which lies at the root of all mathematical philosophy. In its various forms we recognise it in the conflict of Continuity and Discontinuity, of Space and Number, of Geometry and Arithmetic. On being first introduced to the Dedekind " cut " one feels that however logically necessary the apparatus may be yet it is nevertheless arbitrary and even mystifying. After working away at the new ideas for some time most students acquire a certain facility in their manipulation and consequently imagine that they have mastered them, whereas, in most cases, all which they have really acquired is the " outward " technique and not the " inward " content. A little historical knowledge is very helpful here if it

enables us to perceive that the limitations of logic give rise to that conflict of geometry and arithmetic, which is apparent whenever these two coalesce.

We learn how the Pythagoreans evolved a geometric arithmetic so that ultimately all numerical symbols as such disappeared, whereas we, conversely, have evolved an arithmetic geometry and so have at last banished all traces of geometrical intuition from our "rigorous" analysis. The tacit assumption which underlies the apparent conflict is that we, somehow or other, can apply arithmetic to space—in fact, that we can set up a correlation between points and numbers, a 1, 1 [or more generally an m, n] correspondence. A comprehensive appreciation of this assumption and of its implications is ultimately essential to a full understanding of the nature of pure mathematics. Without taking historical considerations into account this appreciation cannot be achieved.

It might well be said that we live in an Age of Applied Mathematics [using the term in the wider as well as in the narrower sense]: not only is there a distinct mathematical influence in technology, but also in the arts. The dominating art is architecture, the most mathematical, and as Mr. Wilenski would have us believe (*vide* "The Modern Movement in Art", etc.), the fundamental art. Cubist painting in France (Picasso), Swedish silver and pewter ware, and in our own country the furniture designed by Mr. Romney Green are obvious instances of the tremendous influence of Geometry on modern taste. To-day, indirectly at least, pure mathematics is exerting a considerable effect on the development of culture. This effect is reciprocal, and no one loses more than the mathematician by any attempt to deny this reciprocity. Do mathematicians as a whole (and scientists too for that matter) yet appreciate the cultural significance of their pursuits? I do not think that Léon Brunschvicg overstates his case when he asserts that "From lack of official institutions in harmony with a general survey of human knowledge, they are, for the most part, left in ignorance of the history of science. . . . The study of the past seems to be left to lovers of phrases, to devotees of the Verbum oratio who can only conceive a superficial and almost grotesque representation of human nature, but whose influence, preponderant in assemblies which are governed by words, directs our education in a way contrary to the needs of civilization. . . . Better informed by their own history, the future representatives of Science will understand, and will make those around them understand, that those alone whose works witness to the sincerity of their attachment to the Verbum ratio are the lawful heirs of the Hellenic wisdom in its true and most truly beautiful form."

GERALD J. WHITROW.

GLEANINGS FAR AND NEAR.

869. Fanny: Well, it's very important. She teaches real things—Mathematics and Science.—C. L. Anthony, *Autumn Crocus*, Act i.

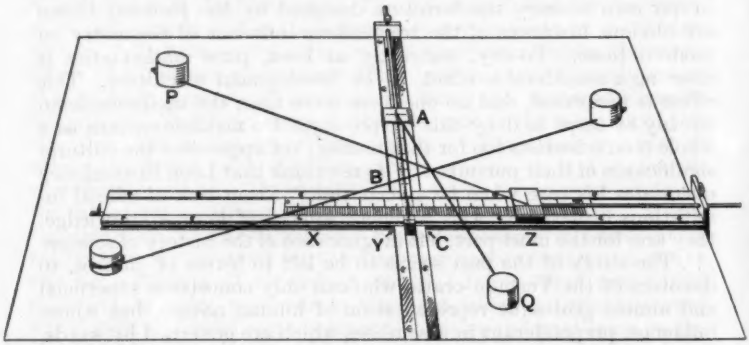
A SCALE CHANGE CALCULATOR WITH THREE CONTROL POINTS.

By H. G. Green, M.A.

A difficulty which is frequently met in dealing with examination results is that of bringing to a common standard the marks obtained from two different papers. The most common form of variation occurs when the mark distribution curves are correct in general character but the peaks are at different points of the scale. We describe a machine devised by the author which has been used for the adjustment of marks. Its theory has no statistical justification, but where the distortions are not excessive the results do not differ appreciably from those of the rather lengthy theoretical methods and are usually absorbed in the approximation of a fraction to its nearest unit. The method uses the 1, 1 transformation

$$\frac{m-a}{m-b}\cdot\frac{c-b}{c-a}=\frac{m'-z}{m'-y}\cdot\frac{x-y}{x-z},$$

where m is the mark on the original scale and m' is the mark on the new one, and three fixed control marks a, b and c are converted into new fixed control marks z, y and x.



The instrument consists of a wooden scale AC and a glass scale XZ which slide in brass grooves at right angles and screwed on a wooden board, the glass scale being above the wooden one. In the model shown the board is 2 ft. 6 in. by 2 ft. 3 in., and the scales are both 1 foot long. On each scale there is a line parallel to its length which bears graduations over the desired range (0 to 120 has proved a convenient standard range). Two brass cursors A, Z with similar graduations to replace the parts of the scales which they cover can be set at desired points of the scales and clamped by means of the rods and screws provided. Cords from A and Z have their other ends attached to movable weights by means of which they are kept stretched. A third cord is kept stretched so as to

cut the scales at X, B by means of weights at both ends. A spring is inserted between each cord and its weight to prevent breakage of the cord. The cords should be fine and of such material that they will not catch on one another. Recesses are cut in the board and provided with straps to hold the weights securely when the instrument is not in use. For simplicity the springs and recesses are omitted from the figure.

We suppose that it is desired to set the machine so that marks

a,	b,	c	become	z,	y,	x	respectively.
87,	30,	13		97,	48,	15	
wooden scale				glass scale			

(1) Set the wooden scale so that C (graduation 13) lies on the graduation line of the glass scale; set the glass scale so that Y (graduation 48) lies on the graduation line of the wooden scale.

(2) Set the cord with weights at both ends to pass (nicely taut) through B (graduation 30) of the wooden scale and X (graduation 15) of the glass scale.

(3) Placing the two remaining weights so that the cords will not be stretched unduly, set the cursor A at graduation 87 of the wooden scale and the cursor Z at graduation 97 of the glass scale. (In the model the cursors were 10 graduations wide, so to make these settings the edges were placed 5 graduations from the graduations desired.)

To find the mark corresponding to any given mark p the weight P is moved with the left hand until the cord PZ cuts the wooden scale at graduation p. The weight Q is then moved with the right hand until the three cords are concurrent. The desired reading q is then given by the point of crossing of the cord AQ and the glass scale.

H. G. GREEN.

University College, Nottingham.

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870. When Pappus had finished his investigations he must have felt that, apart from minor extensions, the subject [conic sections] was practically exhausted. And if he could have foreseen the history of science for more than a thousand years it would have confirmed his beliefs. Yet in truth the really fruitful ideas in connection with this branch of mathematics had not yet been even touched on and no one had guessed their supremely important applications in nature. No more impressive warning can be given to those who would confine knowledge and research to what is apparently useful, than the reflection that conic sections were studied for eighteen hundred years merely as an abstract science, without a thought of any utility other than to satisfy the craving for knowledge on the part of mathematicians, and that then at the end of this long period of abstract study, they were found to be the necessary key with which to attain the knowledge of one of the most important laws of nature.—A. N. Whitehead, *An Introduction to Mathematics.* [Per Mr. F. Fox.]

871. The general effect of the success of the Differential Calculus was to generate a large amount of bad philosophy centring round the idea of the infinitely small. The relics of this verbiage may still be found in the explanations of many elementary text-books on the Differential Calculus. It is a safe rule to apply, that when a mathematical or philosophical author writes with a misty profundity, he is talking nonsense.—*Ibid.* [Per Mr. F. Fox.]

INITIATIVE IN HIGH SCHOOL MATHEMATICS: INVESTIGATIVE EXERCISES.*

By P. N. Andersen, B.A., B.Sc.

I am afraid that what I am going to place before you to-night will be somewhat incomplete. This is unavoidably so because I have not had sufficient time at my disposal to test my views thoroughly. The evidence which I have to offer, therefore, in support of my case is necessarily scanty, and it would be premature to force unwarranted conclusions from it. Moreover, what little testing I have done has been confined almost wholly to one special problem. On that account there is a danger that my presentation of the topics will appear to lack proportion. I hope, therefore, that you will bear these facts in mind when judging this paper, for otherwise your verdicts could not be altogether fair.

Probably all of us have noticed at some time or other that many pupils are oppressed by a sense of the futility of their mathematical studies, particularly in geometry. This may be ascribed to many causes, but certainly one of them is that the exercises themselves are for the most part futile. They invariably take the form : " Given A, prove B " ; and, having proved B, the pupils feel that they are little better off than before. Quite frequently, after helping to complete an exercise, I have been asked the question : " How was that found out in the first place ? " Or again : " What is the use of that ? " Moreover, I have noticed that a result which a pupil has discovered for himself invariably gives him a greater measure of satisfaction than the mere tracking of an inference which has been made previously by someone else. Doubtless, to most of us the solution of a geometrical exercise is an absorbing occupation, and we come in contact with many pupils who have the same leanings ; but the interest of the question is largely that of the puzzle. We are given a mathematical picture ; we are told the thing that is to be found ; puzzle : find it ! But the continued solution of mathematical puzzles will not make mathematicians of us. We must distinguish between mathematics as a pastime and mathematics as a science. The " real " mathematician, as apart from the puzzle-solver, is rarely faced with the answer to his question. He has to scrutinize his data and from them build up possible results, or suggest probable relations, the truth of which can be tested by analysis. The keynote of our mathematical teaching, therefore, should be original investigation, and the pupil should occupy the position of a research student rather than that of a puzzle-solver. In the course of this paper I will make some suggestions with regard to our teaching methods, and if you feel disposed to criticize them, I would ask you to bear this in mind : the results I quote indicate the pupils' work only, without any hints on my part, and a just comparison can be made only if we remember that comparatively

* The substance of an address delivered before the Sydney Branch of the Mathematical Association.

few pupils get their ordinary geometrical exercises out without help in some form or other.

We teachers are being continually reminded of the fact that many of our pupils possess considerable latent initiative. I have noticed instances of this where boys have been searching for results, and again instances where they have devised striking methods of proof. The most noteworthy example of all occurred recently when a pupil * during his second lesson on the integral calculus suggested its employment for the evaluation of π. He carried out an original investigation on these lines : the area of a circle of radius r had been proved trigonometrically to be πr^2, and the area of a circle of unit radius was therefore π ; but, using Cartesian coordinates, and considering the first quadrant, he argued that the area should be $4\int_0^1 (1-x^2)^{\frac{1}{2}}dx$; thus he arrived at the result $\pi = 4\int_0^1 (1-x^2)^{\frac{1}{2}}dx$, and this he evaluated by expanding and integrating term by term. The value found was not very accurate on account of the slow convergence of the series, but it was sufficiently close to the true value to afford him the satisfaction of seeing his own ideas yielding results.†

Most of us could, I think, furnish similar examples of the pupils' initiative seeking to give itself expression. Now, initiative is an inborn quality, and no amount of training will develop it where it does not already exist ; but our teaching is faulty if it does not provide the training appropriate to the development of that faculty to its utmost. It is precisely in this respect that many of our mathematical exercises are defective. We give much practice in questions of the kind that merely ask the pupil to show how a certain result is obtained, but we do not give him an opportunity to reveal his own initiative in seeing what type of result is likely to follow from the data, or of actually deriving his own results. Exercises of the usual type (given A, prove B) eventually become mechanical. Of course, they, too, demand initiative, but not of the kind that is going to be useful in applications of the subject, with one important exception. I do not wish to give the impression that I favour the abolition of questions of the stereotyped kind ; I do think, however, that they should be supplemented. They are very important. In particular they teach us to discriminate between useful and useless data ; but only when the actual or probable result is known. They do not give experience in forecasting a *likely* result, and that is exactly what mathematicians have to do, unless they happen to be teachers, bound by custom, whose apparent *raison d'être* is simply to produce more teachers.

In accordance with this line of thought it was decided to set, from time to time, exercises of a different kind, and these, for want of a better name, I call investigative exercises. Their principal characteristic is that the final result is unknown, though in working the

* Aged 17 yrs. 1 m. Average age of class, 17 yrs. 2 m.

† Four other instances were quoted.

exercise there may or may not be a definite object in view. It is convenient to classify them according to three well defined types.

In Type A certain data are supplied, but there is no definite aim in the form of a specified mathematical result; the pupils are to ascertain from the data every possible additional piece of information. As an example of this kind the following exercise was set (Fig. 1): *

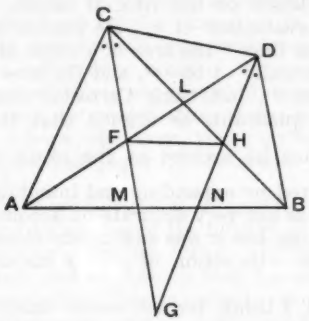

FIG. 1.

"ACB and ADB are two equal angles on the same side of AB. The bisectors of these angles meet AD at F, CB at H, AB at M and N, and intersect at G. CB cuts AD at L and CD, FH are joined. Find what you can from this diagram."

The smallest number of inferences from any one pupil was 5, while the greatest was 40, very trivial results being excluded. The class average was 13. The above diagram was actually taken from an exercise in a text-book, where the pupil was asked to prove that $GF \cdot GC = GH \cdot GD$ and FH is parallel to AB. Both of these were among the results handed in by the class; the first was given by three boys and the second by nine, though many of the inferences recorded were of even greater difficulty than these if set as separate exercises.

Two other exercises of a similar character were set. These were as follow:

"AB is a chord and AD a tangent to a circle at A. DPQ is drawn parallel to AB meeting the circle in P and Q. Join AP, AQ, QB." (Fig. 2.)

"Two circles touch internally at A and a chord PQ of the larger circle touches the smaller at R. PA and QA cut the smaller circle at X and Y respectively. Join RA, XY." (Fig. 3.)

In each case the pupils were to ascertain from the data every relation they possibly could. To aid them in this they were allowed to employ any additional construction they found necessary, but the final relations were to refer only to the diagrams as given.

* To a fourth year class, average age 16 yrs. 1 m.

The smallest number of inferences was 1 in the first and 0 in the second. The figures for the greatest number were 28 and 34, while the averages were 10 and 9. The results of these exercises have been graphed, y-axis measurements representing the number of inferences recorded by any one pupil, and x-axis measurements the positions of the pupils (see Figs. 4-6). The following points are worth noticing:

(i) In quite a reasonable number of cases the pupil on his own initiative devised a result of standard equal to that of an ordinary exercise. Sometimes even the actual result as it appeared in the text-book found its place among the recorded inferences.

(ii) There is a fairly steady gradation in the number of inferences recorded in each case, and this number, therefore, may be taken as a rough measure of the pupils' success.

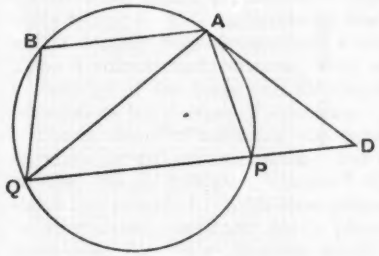

FIG. 2.

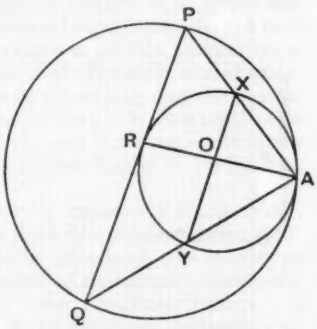

FIG. 3.

(iii) The exercises are of increasing difficulty, and this is indicated by a drop in the left-hand end of the graph. It is interesting to see that while the weaker boys were easily overcome, the better boys were not very seriously affected by the increased difficulty; in other words, they proved their ability.

(iv) Considering the order of arrangement of those boys who worked all three exercises, I found that there was a tendency to uniformity, though some varied greatly in their relative positions. It would be absurd to expect the order to be quite definite; but the grading according to the averages agreed fairly closely with my personal impression, while it differed considerably from that on the half-yearly reports which was based on the results of class tests involving questions on theorems and exercises of the usual type.

In order to examine this grading more closely a class test was given including two questions of Type A, twenty minutes being allowed for each. Again the result given in the text-book appeared among the inferences, and again the graphs show a distinct gradation in the number of inferences recorded. The final graph (Fig. 7) shows the averages over all five exercises, and it is interesting to note that for this range there is a fairly definite ranking. Conclusions drawn

from these results, however, would not necessarily be reliable, partly because of the restricted range of the experiments and partly because the number who gave results for all five was considerably affected by irregularity in attendance. The 24 concerned have been arranged in order of merit according to the averages, and are identified by the letters $A, B, C, \ldots X$ in ascending order. The positions of the same 24 according to the half-yearly reports is $B, C, J, T, F, O, K, N, Q, A, D, \{E,S,W\}, M, H, I, X, \{R,G\}, P, L, V, U$. There is thus a marked discrepancy between the two rankings, the former being in close agreement with my own personal impression of the boys. It is possible, of course, that if the number of tests of each kind was increased, the two rankings would be more nearly the same.

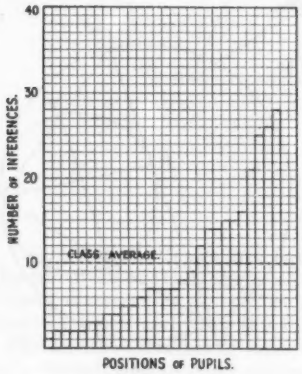

FIG. 4.

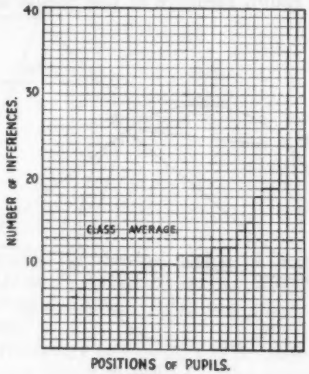

FIG. 5.

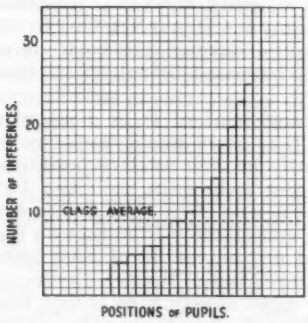

FIG. 6.

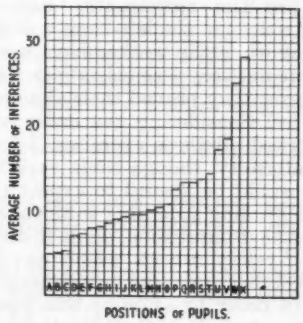

FIG. 7.

For those who are interested in correlation coefficients the following results have been obtained employing the formula $r = \dfrac{\Sigma(xy)}{N\sigma_x \cdot \sigma_y}$ in connection with the actual numbers of inferences recorded. Exer-

cises 1 and 3 give $r = \cdot 70$; 4 and 5 give $\cdot 59$; 2 and 3, $\cdot 61$; and correlating the averages for the first three (worked at home) with the averages for the two worked as a single class test gives $r = \cdot 77$.

A more important consideration than accurate ranking, however, is the allocation of marks in just proportion to examination candidates for what they have actually done. Unless a candidate obtains a complete solution to an exercise, he usually hands in no working, even though he may have reasoned to within a step or two of the final solution. Many examinees are severely and unnecessarily penalized on this account. Of course we must test our pupils on definite problems, but the marking difficulty could to some extent be remedied by setting some exercises of Type A. Such questions could conveniently replace those which ask for reproduction of bookwork. The place for testing knowledge of bookwork is surely the classroom, and I cannot see any educational significance in the fact that a boy either can or cannot reproduce a certain section on a specified day under examination conditions. The inclusion of questions asking for this undoubtedly leads to cramming, and the examination largely degenerates into a memory test. Were exercises of Type A substituted, however, they would test both the candidate's knowledge of the facts and also his understanding of those facts as revealed in his powers of inference.

The problem of assessing the number of inferences is one which requires careful consideration ; but it does not present undue difficulties. With first year classes * clearly inferences of every type should be recorded. With later years some of the simpler ones could be eliminated, emphasis being placed upon *significant* results. A good plan for older students would be to limit the inferences to certain types, *e.g.* equal angles, similar triangles, equal ratios, relations between areas, and so on.

The advantages which follow the use of exercises of Type A are fairly obvious, but the following points may be specially noted : firstly, they give the teacher a deeper knowledge of the capabilities of each individual pupil, and, secondly, they stimulate the pupils' interest and lead easily to the *need* for an extension in their theory.

In connection with the stimulation of interest, the following exercise had my present first year class † thoroughly excited :

"The equal sides BA and CA of an isosceles triangle are produced to D and E respectively making $AD = AE$. Join BE, CD." (Fig. 8.) The text-book required a proof that $BE = CD$. Ignoring that part of the question I set the boys *looking* for results, and in the course of no longer than five minutes they discovered not only that equality, but numerous other facts including congruent triangles, isosceles triangles, equal lengths, equal angles, and we concluded by proving that FA produced bisects BC at right angles, where F is the point where BE and CD meet when produced.

On another occasion the data and construction of Euclid I. 16 were taken as the starting point for an investigation (Fig. 9). The

* Average age 12½-13 yrs. † Average age 13 yrs.

class soon arrived at the conclusion that $\angle ACD > \angle BAC$. They were asked for suggestions as to how it could be proved that

$$\angle ACD > \angle ABC.$$

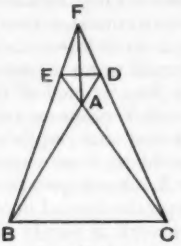

FIG. 8.

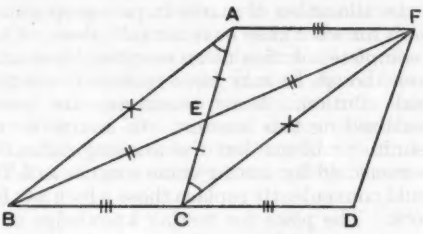

FIG. 9.

The ingenious construction of the diagram was given and they sought to prove the triangles ABC, FCD congruent, thereby showing that $\angle FCD = \angle ABC$ and so $\angle ACD > \angle ABC$. Of course their attempts broke down through their ignorance of the theory of parallels. This was the second occasion on which their efforts had been hampered by the same deficiency; but these failures made quite clear to them the limitations of their present knowledge and consequently they are eager to commence the study of parallels.

The purpose in exercises of Type B is more specific. It is intended that free scope should still be afforded the pupil for training his own ideas, but his efforts are given more direction in that he is confronted with a definite problem, though the result of his quest is to him unknown. A typical illustration is the locus problem where the locus is not indicated in the question. Here are two other examples:

(i) "In four concurrent straight lines OAP, OBQ, OCR, ODS, the points are so chosen that AB is parallel to PQ, BC parallel to QR, and CD parallel to RS. Investigate the directions of AD and PS."

(ii) "ABC is a triangle in which BE and CF are drawn perpendicular to AC and AB respectively and intersect at O. AO is joined and produced to meet BC at D. Join DE, EF, FD. How many cyclic quadrilaterals can you find in the diagram? What is the size of $\angle ADB$?"

Exercises of this type can be made a very suitable preliminary to work on new theorems. Where the final result is difficult the questions can be made inductive in character so that the nature of the result may be more readily suggested.

It may be said, in passing, that geometry seems to offer greater opportunities for investigative exercises of all types than either trigonometry or algebra. A suitable opportunity offers itself, however, in the treatment of identities in trigonometry. In later stages of the subject, and in applications to other sciences, our usual problem seems to be to reduce a given expression to a simpler form, or to render it accessible to treatment by logarithms; but the final result is left to our own devising. In the face of this need our text-

books give copious sets of exercises in which the pupil is asked to prove that expression A is equal to expression B. The disadvantages are twofold: firstly, such exercises do not sufficiently train one to deal satisfactorily with those problems where an *original* simplification is demanded, for one usually looks to the answer as a guide; and, secondly, they prejudice the pupils on account of their patent uselessness. What little work I have done in this direction has convinced me both of the pupil's ability to simplify a given expression without being supplied with directions in the form of an answer, and also of his increased interest in his work when his own ingenuity is being put to the test.

Here is a Type B exercise from the series work in algebra: *

"Examine the following sets of numbers:

$1+2+3$; $1+2+3+4+5$; $1+2+3+4+5+6+7$; $1+2+\ldots+9$.

Make what observations you can. If you so desire, write down a few more similar sets. Could you devise a quick method for adding up each set? Could you apply it to the set $1+2+3+\ldots+1001$? Could you apply it to the set $1+2+3+\ldots+n$, where n is odd?"

This exercise was purely inductive in character, and its value obviously lay in the fact that it was given prior to any work on the progressions. It partook of the nature of an experiment and on this account the interest shown was intense. The observations made were interesting and the methods of summation varied. A considerable number stated that the sum was "the number of numbers × the middle number" and that the middle number was $\frac{1}{2}$(first + last); others that it was "the last number × the middle number". One arranged in groups, counting 1 as the first group, and said that the sum was "the last number × the number of the group". Another gave the result as "last number × m, where to get m we count back from the last term a number of terms equal to the number of the group (counting $1+2+3$ as the first group)". Finally, one gave it as "$\frac{1}{2}$(first + last)^2 − the middle number". In whatever form the rule was given, it led in most cases to the result $\frac{1}{2}n(n+1)$. Further work showed the formula to be true when n was even; but the rule had to be altered till it took the form "number of numbers × average number". An attempt was then made to prove the formula $\frac{1}{2}n(n+1)$ without relying on induction. This proved too difficult on account of the general nature of n. A full proof, based on their rule, was given them and finally the result was proved by the usual method of reversing the order of the terms. Their appreciation of the neatness and simplicity of this proof was immediate, and depended largely on the difficulty experienced in following their own line of thought. I am convinced that if a pupil has himself experienced a sense of frustration in seeking a result, or even if he has obtained it after considerable labour, he is in an ideal position to appreciate the elegance of a simpler though less obvious method which has not occurred to him. Some of our students' difficulties, I think, are due to the fact that we present

* Set to fourth year.

to them methods which have been perfected by time, and which have therefore lost some of their original directness. I have known cases where pupils have actually followed the argument step by step but have not followed it as a whole, simply because the reason for that particular line of thought has not been manifest. Experiences of this kind must eventually impress the pupil with a sense of his own impotence and foster a dislike for mathematical studies. The cure seems to be to let him grapple with some problems himself, and, by trying his own strength, learn to appreciate the weapons of some other person's devising. Moreover, in his statement of the result of his investigation, he invariably shows what difficulties he has in groping after the essentials of a problem and in making accurate and concise statements. The experience he gets by his own efforts, therefore, followed by the teacher's work in placing things in correct perspective, should be of inestimable value to him.

My principal object in setting exercises of Type B, however, was to try to develop greater self-confidence in my pupils, particularly in geometry. Let me assert again that the usual geometrical exercise has its place; it cannot be dispensed with. But its *exclusive* use tends indirectly to both lack of self-confidence and intellectual dishonesty. When completing an exercise, many boys base their satisfaction solely on the fact that someone else has previously arrived at the same result. How often are we faced with the position that a boy is quite satisfied with his answer merely because it agrees with that of the text-book! I have even seen reasoning fabricated for the sake of securing this agreement; and there is a most decided tendency for pupils to deduce the accuracy of their own argument from this agreement rather than from confidence in their own logic. The tendency to make the logic fit the conclusion must be well known to examiners, but it is just as evident in the classroom. With the investigative type of exercise their assurance is not that which is engendered by the fact that someone else has blazed the trail before them. They are dependent on self and must satisfy their own minds as to the accuracy of their logic. More careful reasoning, greater self-confidence and assurance, and the habit of looking for possibilities should follow, and these are the very qualities we wish to develop.

The following two exercises were recently set my fourth year class:

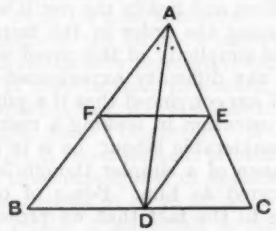

FIG. 10.

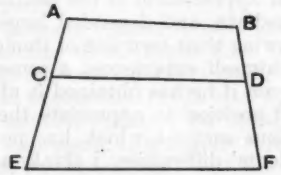

FIG. 11.

"*ABC* is a triangle in which *AD* bisects $\angle BAC$ and meets *BC* at *D*. *DE* and *DF* are drawn parallel to *BA* and *CA* meeting *CA* and *BA* in *E* and *F* respectively. It is thought that perhaps *FE* is parallel to *BC*. Can you decide the question? Also determine the ratio *BF*/*CE* in terms of *AB* and *AC* alone (Fig. 10)."

"*AE* and *BF* are two straight lines cut in the same ratio at *C* and *D*. Are *AB*, *CD*, *EF* parallel (Fig. 11)?"

Questions of this kind have an added value in that they give pupils an opportunity of demonstrating the *un*truth of certain statements as much as the truth of certain others. Admittedly they are more difficult than the ordinary exercise, but the matter of difficulty is one that is quite within our own control. The two exercises given above should surely not be beyond the reach of pupils of fourth year high school standard. Yet here are the results. Written work was handed in by 23 boys. In the first exercise 2 gave the correct result and proved it; 9 gave the correct result with either inadequate or incorrect reasoning; 11 gave the wrong result and an argument purporting to prove it; while 1 was unable to come to any conclusion. No pupil gave the correct result for the last part of the question. In the second exercise 3 gave the correct result and demonstrated it; 9 came to the right conclusion but their reasoning was either inadequate or faulty; 1 was wrong; while 10 were unable to decide the question.

These figures are enlightening. In going over the work handed in one could not but be impressed by its inaccuracy, by the uncertainty that dogged its logic, and by the fact that in some cases the conclusion seemed to have preceded the reasoning. I do not think this can be attributed to difficulty in the questions. It is due to a total lack of training in work of this kind and to a resultant hesitancy and lack of independence on the part of the pupils when called upon to think for themselves, for, under the present system, there is always the tendency to *prove* a result irrespective of whether it is right or wrong.

Here is a more recent example: "*ABC* is a triangle in which *AB* is greater than *AC*. On what side of the triangle will the external bisector of $\angle BAC$ again cut the circumcircle?"

On this occasion the figures show an improvement: 8 gave a completely satisfactory solution; 6 had the correct result but faulty reasoning; 4 were totally wrong; and 10 could arrive at no solution. I feel sure that with further training the figures could be steadily improved. Judging by the latent initiative that has forced itself upon my notice from time to time I do not see any reason why pupils should not be as successful at investigative exercises as at any other kind, provided they are trained in them from the beginning and the teacher makes a suitable choice of material. My firm conviction is that exercises of Type B are very necessary in a well-balanced mathematical education, for our pupils must have ample opportunities for developing their own initiative and attaining a fair measure of assurance in reasoning on their own.

Type C exercises make a further call on the pupil's originality.

Here he is not even supplied with data but is asked to invent an exercise of his own. This is a decidedly more difficult task and makes a considerable demand on his inventiveness as well as on his insight into the possibilities of various types of diagrams. On account of this difficulty, exercises of this kind should not be set frequently. Probably once a month would be often enough.

I have set two such tasks to each of my first and fourth year classes. A comparison of results is wholly in favour of the first year boys who have been encouraged in every way to think for themselves. Many of the first attempts from fourth year were not only based on first and second year work, but were merely of first or second year standard; and even when the data given promised reasonably good results there was a general lack of confidence in continuous reasoning. A few of the first year efforts showed originality of a distinctly high order even though the pupils had much less material on which to work. A characteristic fault with both classes was an over-sufficiency in the data supplied. For that very reason original work of Type C should be invaluable. With tactful handling by the teacher it should lead to a greater appreciation of the exact significance of the data of a question, and further it should give excellent practice in the exact statement of mathematical facts. Admittedly some of the work was of very poor quality, but the best was decidedly encouraging. Here are a few of the *best* examples (I have remodelled some of the statements).

I. From fourth year:

(1) "$ABCD$ is a square. On AB as hypotenuse describe a right-angled triangle AEB. Produce EB to F so that CF is perpendicular to BF. Join FA. Prove that AF cannot bisect $\angle DAE$." (Fig. 12.)

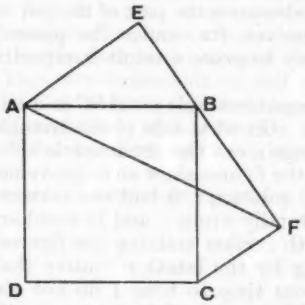

FIG. 12.

(2) "ABC is a triangle right angled at A, and AD is drawn perpendicular to BC. The bisectors of $\angle ABD$, $\angle DAC$ meet AD, DC in X, Y respectively. Prove that $AD \cdot XD = BD \cdot DY$." (Fig. 13.)

(3) "Two chords AB and CD of a circle are produced towards B and D to meet at E. Through E a parallel is drawn to DA meeting CB produced at F. Prove that $EF^2 = FB \cdot FC$." (Fig. 14.)

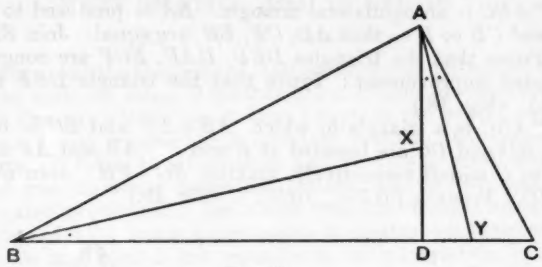

FIG. 13.

II. From first year :

(1) "*ABCDE* is a regular pentagon. *AC* and *AD* are joined. Prove $\angle ACD = \angle ADC$." (Fig. 15.)

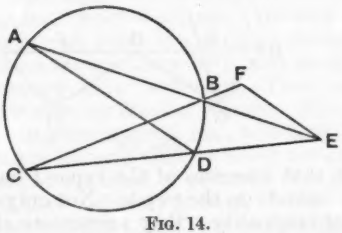

FIG. 14.

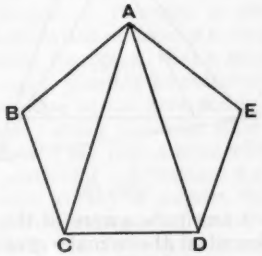

FIG. 15.

(2) "*ABC* is an isosceles triangle in which *D* is the mid-point of the base *BC*. From *AB* and *AC* cut off *AX*, *AY* of equal length; from *XB*, *YC* cut off *XP*, *YQ* of equal length. Join *XD*, *PD*, *YD*, *QD*. Prove $\angle XDP = \angle YDQ$." (Fig. 16.)

(3) "*ABC* is an equilateral triangle. *BA* and *CA* are produced to *E* and *D* respectively so that *AD* = *AE*. *BC* is produced both ways to *F* and *G* so that *FB* = *CG*. Join *DF*, *DB*, *EC*, *EG*. Prove *DF* = *EG*." (Fig. 17.) (Note the unnecessary data.)

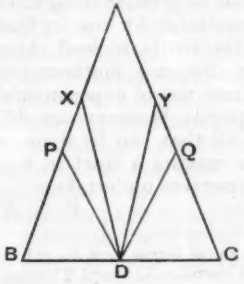

FIG. 16.

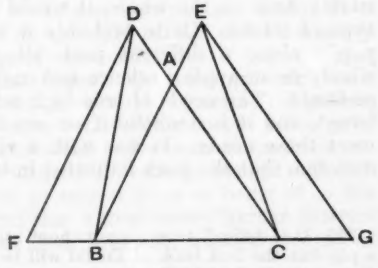

FIG. 17.

(4) "ABC is an equilateral triangle. BA is produced to D, AC to F, and CB to E so that AD, CF, BE, are equal. Join ED, DF, EF. Prove that the triangles DEB, DAF, ECF are congruent." (Suggested improvement: Prove that the triangle DEF is equilateral.) (Fig. 18.)

(5) "ABC is a triangle in which $AB=AC$ and BC is bisected at D. BD and DC are bisected at E and F. AE and AF are produced to G and H respectively, making $EG=FH$. Join BG, GD, DH, HC. Prove $\angle BGD=\angle DHC$." (Fig. 19.)

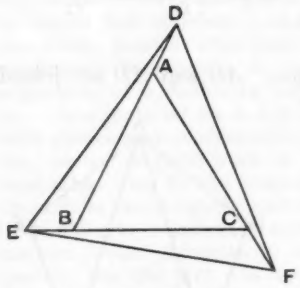

FIG. 18.

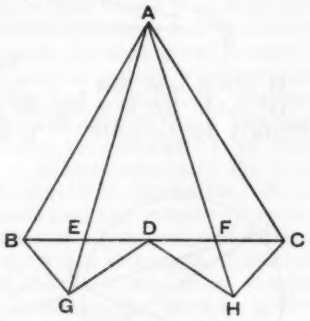

FIG. 19.

I am quite aware of the fact that exercises of the types I have described above make greater demands on the pupils. Not only do they require a larger measure of originality; they necessitate also greater accuracy in thinking. But I am most emphatically of the opinion that they are not too difficult. I am convinced that our class work should give considerable training on these lines if we are to interest our pupils fully and acquaint them thoroughly with the uses and powers of the subject.

In conclusion let me stress one point. The title of my address referred to initiative in high school mathematics, and it is precisely the genuine high school pupil who requires training along the lines suggested. There are, perhaps, some pupils in super-primary classes of other types of schools who could with profit be given investigative work; but, on the whole, it would be unsuitable for use in that type of school. Quite probably it would be better to lead those pupils along a different path altogether, treating mathematics wholly as an applied science and making freer use of experimental methods. The needs of true high school pupils, however, are different, and it is doubtful if we are doing all that can be done to meet those needs. It was with a view to making a start in this direction that the work indicated in this paper was undertaken.

P. N. A.

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872. It is difficult to explain without using technical terms, how to cut up a pig, but the first book of Euclid will be found useful.—Leonard Fleming, *Fun on the Veld*, p. 157.

MATHEMATICS AND CULTURE.*

By J. W. N. Sullivan.

In talking about the relations of mathematics and culture or, rather, about the cultural value of mathematics, I think it advisable that I should first say a little about what I suppose the word "culture" to mean. In its general use it contains, I think, two ingredients, as it were. It refers to information and it refers to sensibility. An ignorant man, however refined his natural sensibilities may be, is not regarded as cultured. And a man who has amassed an immense knowledge of facts, but is quite unable to distinguish between what is valuable and what is not valuable in his information, is not regarded as cultured. Of these two elements, I am inclined to think that sensibility is the more important. What the old writers on aesthetics used to call taste is, I think, more important than knowledge for the formation of a cultured outlook. But the word "taste" as they used it, is too narrow for our purposes. It refers to our aesthetic sensibilities, and doubtless the cultivation of these is a very important part of culture. We would agree, I suppose, that a man who is completely insensible to the arts cannot possibly be cultured. But an acquaintance with, and an appreciation of the arts, though necessary, is not sufficient. There are many other elements which enter into our ideal of culture. Thus I should say that a man with no historical sense, a man who took the particular conventions and ideas of his own time as being the only ones worthy of respect, fell short of the ideal of culture. Mr. Henry Ford is reported to have said that "History is bunk". One can only conclude that, in one respect at least, Mr. Ford is uncultured. Similarly with an intense provincialism. The type of Englishman, who seems to have abounded in the nineteenth century, who regarded all foreigners as in all respects inferior, was what we should call uncultured. In fact, a good deal of what passed, and still passes, for patriotism, is probably only provincialism and therefore due to lack of culture.

Culture is chiefly, I think, the refinement and education of all our sensibilities, including our intellectual and animal sensibilities, and not only of those that are concerned with the arts. Other things being equal, I should say, for instance, that the man who could tell good wine from poor wine was more cultured than the man who could not. There is no need to labour this point. It would be generally agreed, I think, that an essential part of culture consists in the refinement and education of our sensibilities.

And now I come to my main subject. On what grounds can it be asserted that mathematics has a cultural influence? I gather, from various remarks that I have come across in the writings of various authors, that mathematicians have sometimes been regarded as almost the least cultured of human beings. Mathematicians have sometimes been presented to us as being of so dry a nature and so warped a mind that almost every human interest is alien to them. Even James Clerk Maxwell, when a student at

* Presidential Address to the London Branch.

Cambridge, complained that some of the men there saw the whole universe in terms of quintics and quantics and seemed incapable of realizing that the universe had any other aspects. And we have all heard of the mathematician who, on being persuaded to read Milton's *Paradise Lost*, said at the end that he didn't see what the man was trying to prove. Now it must be admitted that there are a good many mathematicians who lend colour to this assertion of insensitiveness. It seems to be possible for a man to have great mathematical ability and yet to be, in other respects, practically a barbarian. It would seem, in fact, that mathematics is a curiously isolated activity. It seems to be able to flourish in almost complete isolation from the rest of the elements of a man's nature. But we shall do well to distrust this conclusion. Psychologists are not disposed, nowadays, to talk about isolated mental faculties. Indeed, some of them have gone to the other extreme, and deny the existence of any special abilities at all. A genius, according to them, is simply a man with an abnormal amount of general ability. What line his genius takes is dependent on his circumstances. I find it difficult to go as far as this. I find it difficult to believe that, if their circumstances had been interchanged, Napoleon would have composed the Ninth Symphony and Beethoven would have won the Battle of Austerlitz. But although I am disposed to believe that there is a special mathematical ability I am not disposed to believe that it exists in complete independence of everything else. I think we must admit, however, that it is more isolated than some other special abilities.

Nevertheless, can we claim for mathematics any general cultural influence ?

Let us ask, in the first place, what sort of information is given us by mathematics, and as this enquiry will tell us something about the nature of mathematics, I shall devote some time to it. At the beginning, when this mysterious activity was still young, it was supposed to give us the most valuable of all information. It was supposed to reveal to us the ultimate principles on which the universe is constructed. To the Pythagoreans every mathematical theorem had a mystic significance. Number, it was thought, is the very essence of the real. Other aspects of the real which do not, at first sight, seem to have anything to do with number, were found by the Pythagoreans to be, after all, only aspects of number. The analogical method they used to establish this conclusion seems to us, nowadays, remarkably obscure. Thus they found that reason is merely an aspect of the number one. For what is the great characteristic of reason ? According to the Pythagoreans the great characteristic of reason is its unchangeableness, and nothing, they said, is so unchangeable as the number one. This sounds unconvincing to us, for the number one seems no more unchangeable than any other number. But the Pythagoreans must have thought there was a difference, for they go on to say that the number two is indeterminate. " Opinion ", therefore, as contrasted with reason, is an expression of the number two. Two is also, it appears, a feminine number, whereas three is a masculine number. Thus the very essence of

marriage is expressed by the number five, for it is the sum of the first masculine and the first feminine number. This way of thinking seems very strange to us, but we can agree, I think, that it must have made the study of arithmetic a very exciting pursuit. We must remember that many good minds thought in this way, and that it is a way of thinking that persisted for several centuries. Indeed, I am not sure that it is even now altogether extinct. St. Augustine, for instance, found that God created the world in six days because six is the perfect number. The number six is perfect because it is the sum and also the product of the first three numbers. A number with such remarkable properties is obviously perfect. Michael Stifel, the great German algebraist of the sixteenth century, changed his faith because he found that the numbers corresponding to the letters making up the name of Pope Leo the Tenth added up to 666, the number of the beast in the book of Revelation. This mathematician also, by applying a similar method to certain Biblical writings, found that the world was due to come to an end on 3rd October, 1533. Many peasants of his district, who believed him, abandoned their work, with the result that when the day came and passed, they found themselves ruined. This might surely be taken as an experimental proof that numbers did not possess the significance they were supposed to have. But the attitude persisted. Thus even Kepler, after demonstrating the mathematical properties of star-polygons and such-like figures, goes on to explain their magical properties in warding off diseases and other misfortunes. Indeed, Kepler's fundamental outlook was not unlike that of the Pythagoreans. He not only thought that Nature obeyed mathematical laws, but he thought that the reason for Nature's existence was that it illustrated these laws. This point of view is perhaps not unlike the philosophy of God the Pure Mathematician that Sir James Jeans has recently expounded to us. It would seem possible that Pythagoreanism still flourishes, not only in such beliefs as that 13 is an unlucky number, but in more rarefied forms. In its lower forms it certainly still flourishes. Quite recently, before the draw for the Irish Sweep took place, I encountered a man, in the local inn, who insisted on standing everybody a drink because he had drawn a winning ticket. His reason for thinking this was that the figures on the ticket added up to a number which had brought him good fortune throughout his life. Charles Dickens, you remember, was much impressed by the constancy of the yearly averages for deaths by railway accidents. If, by the end of November, he found the figures much behind, he was greatly depressed at the thought of the slaughter that was bound to take place the next month, and took care not to travel. Doubtless we have all met with several instances of the same kind of thing. Another, and more respectable aspect of Pythagoreanism, is the doctrine that mathematics acquaints us with necessary truths.

The point of view is well put by Descartes in a famous passage from his *Fifth Meditation*:

"I imagine a triangle, although perhaps such a figure does not

exist and never has existed anywhere in the world outside my thought. Nevertheless this figure has a certain nature or form or determinate essence which is immutable and eternal and which I have not invented and which depends in no way on my mind. This is evident from the fact that I am able to demonstrate various properties of this triangle, for example, that its three interior angles are equal to two right angles, that the greatest angle is subtended by the greatest side and so on. Whether I want to or not, I recognise very clearly and evidently that these properties are in the triangle although I have never thought about them before, and even if this is the first time I have imagined a triangle. Nevertheless, no one can say that I have invented or imagined them."

A triangle, therefore, according to Descartes, does not depend in any way upon one's mind. It has an eternal and immutable existence quite independent of our knowledge of it. Its properties are discovered by our minds but do not in any way depend on them. This way of regarding geometrical entities lasted for two thousand years. To the Platonists geometrical propositions, expressing eternal truths, are concerned with the world of Platonic Ideas, a world apart, separate from the sensible world. The Platonic Ideas exist in a sort of heaven of their own and things on this earth are imperfect embodiments of them. To the followers of St. Augustine these Platonic Ideas became the ideas of God ; and to the followers of St. Thomas Aquinas they became aspects of the Divine word. Throughout the whole of scholastic philosophy the necessary truth of geometrical propositions played a very important part. If this outlook be justified, then the mathematical faculty gives us access, as it were, to an eternally existing, although not sensible, world. Before the discovery of mathematics this world was unknown to us, but it nevertheless existed, and Pythagoras no more invented mathematics than Columbus invented America.

We no longer believe this to be a true description of the nature of mathematics. We no longer believe that mathematics is a body of knowledge about an existing, but super-sensible world. Some of us will be reminded of the claims certain theorists have made for music. Some musicians have been so impressed by the extraordinary feeling of inevitability given by certain musical works that they have declared that there must be a kind of heaven in which musical phrases already exist. The great musician discovers these phrases—he hears them, as it were. Inferior musicians hear them imperfectly ; they give a confused and distorted rendering of the pure and celestial reality. The faculty for grasping celestial music is rare. The faculty for grasping celestial triangles, on the other hand, seems to be possessed by all men.

These notions, as far as geometry is concerned, rest upon the supposed necessity of Euclid's axioms. The fundamental postulates of Euclidean geometry were regarded, up to the early part of the nineteenth century, by practically every mathematician and philosopher, as necessities of thought. It was not only that Euclidean geometry was considered to be the geometry of existing space—it

was the necessary geometry of any space. Yet it had quite early been realized that there was a fault in this apparently impeccable edifice. The well-known definition of parallel lines was not, it was felt, sufficiently obvious, and the Greek followers of Euclid made attempts to improve it. The Arabians, also, when they acquired the Greek mathematics, found the parallel axiom unsatisfactory. No one doubted that it was a necessary truth, but they thought there should be some way of deducing it from the other and simpler axioms of Euclid. With the spread of mathematics in Europe came a whole host of attempted demonstrations of the parallel axiom. Some of these were miracles of ingenuity, but it could be shown in every case that they rested on assumptions which were equivalent to accepting the parallel axiom itself. One of the most noteworthy of these investigations was that of the Jesuit priest Saccheri, whose treatise appeared early in the eighteenth century. Saccheri was an extremely able logician, too able to make unjustified assumptions. His method was to develop the consequences of denying Euclid's parallel axiom while retaining all the other axioms. In this way he expected to develop a geometry which should be self-contradictory, since he had no doubt that the parallel axiom was a necessary truth. But although Saccheri struggled very hard he did not succeed in contradicting himself. What he actually did was to lay the foundations of the first non-Euclidean geometry. But even so, and although D'Alembert was expressing the opinion of all the mathematicians of his time in declaring the parallel axiom to be the "scandal" of geometry, no one seems seriously to have doubted it. It appears that the first mathematician to realize that the parallel axiom could be denied and yet a perfectly self-consistent geometry constructed was Gauss. But Gauss quite realized how staggering, how shocking, a thing he had done, and was afraid to publish his researches. It was reserved for a Russian, Lobachewsky, and a Hungarian, Bolyai, to publish the first non-Euclidean geometry. It at once became obvious that Euclid's axioms were not necessities of thought, but something quite different, and that there was no reason to suppose that triangles had any celestial existence whatever.

We now believe that a mathematical theorem is necessary only in the sense that it is a logical consequence of its axioms and postulates, and that its axioms and postulates are arbitrary.

It appears, then, that there is no world, independent of the human mind, about which mathematics gives us any information. We can start from any set of axioms we please, provided they are consistent with themselves and one another, and work out the logical consequences of them. By doing so we create a branch of mathematics. The primary definitions and postulates are not given by experience, nor are they necessities of thought. The mathematician is entirely free, within the limits of his imagination, to construct what worlds he pleases. What he is to imagine is a matter for his own caprice; he is not thereby discovering the fundamental principles of the universe nor becoming acquainted with the ideas of God. If he can

find, in experience, sets of entities which obey the same logical scheme as his mathematical entities, then he has applied his mathematics to the external world ; he has created a branch of science.

Some writers, who accept this account of mathematics as a purely man-made thing, find it an extraordinary fact that the external universe, which is presumably not a man-made thing, should nevertheless obey mathematical laws. If mathematics is merely a mental game, like chess, why should the universe obey the rules of this game ? This remarkable correspondence has led to far-reaching speculations. As a specimen of them I will quote from Sir James Jeans' recent book, *The Mysterious Universe*. He says : " a scientific study of the action of the universe has suggested a conclusion which may be summed up, though very crudely and quite inadequately, because we have no language at our command except that derived from our terrestrial concepts and experiences, in the statement that the universe appears to have been designed by a pure mathematician.

" This statement can hardly hope to escape challenge on two grounds. In the first place, it may be objected that we are merely moulding nature to our preconceived ideas. The musician, it will be said, may be so engrossed in music that he would contrive to interpret every piece of mechanism as a musical instrument ; the habit of thinking of all intervals as musical intervals may be so ingrained in him that if he fell downstairs and bumped on stairs numbered 1, 5, 8 and 13 he would see music in his fall. In the same way, a cubist painter can see nothing but cubes in the indescribable richness of nature—and the unreality of his pictures shews how far he is from understanding nature ; his cubist spectacles are mere blinkers which prevent his seeing more than a minute fraction of the great world around him. So, it may be suggested, the mathematician only sees nature through the mathematical blinkers he has fashioned for himself.

" A moment's reflection will shew that this can hardly be the whole story. Our remote ancestors tried to interpret nature in terms of anthropomorphic concepts of their own creation and failed. The efforts of our nearer ancestors to interpret nature on engineering lines proved equally inadequate. Nature has refused to accommodate herself to either of these man-made moulds. On the other hand, our efforts to interpret nature in terms of the concepts of pure mathematics have, so far, proved brilliantly successful. It would now seem to be beyond dispute that in some way nature is more closely allied to the concepts of pure mathematics than to those of biology or engineering, and even if the mathematical interpretation is only a third man-made mould, it at least fits nature incomparably better than the two previously tried.

" Fifty years ago, when there was much discussion on the problem of communicating with Mars, it was desired to notify the supposed Martians that thinking beings existed on the planet Earth, but the difficulty was to find a language understood by both parties. The suggestion was made that the most suitable language was that of pure mathematics ; it was proposed to light chains of bonfires in

the Sahara, to form a diagram illustrating the famous theorem of Pythagoras, that the squares on the two smaller sides of a right-angled triangle are together equal to the square on the greatest side. To most of the inhabitants of Mars such signals would convey no meaning, but it was argued that mathematicians on Mars, if such existed, would surely recognise them as the handiwork of mathematicians on earth. In so doing, they would not be open to the reproach that they saw mathematics in everything. And so it is *mutatis mutandis* with the signals from the outer world of reality, which are the shadows on the walls of the cave in which we are imprisoned. We have already considered with disfavour the possibility of the universe having been planned by a biologist or an engineer; from the intrinsic evidence of this creation, the Great Architect of the Universe now begins to appear as a pure mathematician.

"In the second place, our statement may be challenged on the ground that there is no absolutely sharp line of demarcation between pure and applied mathematics. It would of course have proved nothing, if nature had merely been found to act in accordance with the concepts of applied mathematics; these concepts were specially and deliberately designed by man to fit the workings of nature. And it may be objected that even our pure mathematics does not in actual fact represent a creation of our own minds so much as an effort, based on forgotten or subconscious memories, to understand the workings of nature. If so, it is not surprising that nature should be found to work according to the laws of pure mathematics. It cannot of course be denied that some of the concepts with which the pure mathematician works are taken direct from his experience of nature. An obvious instance is the concept of quantity, but this is so fundamental that it is hard to imagine any scheme of nature from which it was entirely excluded. Other concepts borrow at least something from experience; for instance multi-dimensional geometry, which clearly originated out of experience of the three dimensions of space. If, however, the more intricate concepts of pure mathematics have been transplanted from the workings of nature, they must have been buried very deep indeed in our subconscious minds. This very controversial possibility is one which cannot be entirely dismissed, but in any event it can hardly be disputed that nature and our conscious mathematical minds work according to the same laws. She does not model her behaviour, so to speak, on that forced on us by our whims and passions, or on that of our muscles and joints, but on that of our thinking minds. This remains true whether our minds impress their laws on nature, or she impresses her laws on us, and provides a sufficient justification for thinking of the designer of the universe as a mathematician."

What are we to think of this argument? It seems to me that Sir James Jeans does not give sufficient weight to the two objections he imagines being brought against his theory. Both objections are serious. Some mathematical logicians assert, for example, that a mathematical web, as it were, can be woven about any universe

containing several objects. If this be true, then the only evidence for a mathematical designer of the universe is to be found in the fact that the universe consists of several objects. This would seem a very inadequate ground on which to arrive at so far-reaching a conclusion. The second objection has also been made. Mathematics and logic, it is stated, are merely devices of adaptation or accommodation to the world in which we find ourselves. They are ways of thinking that have been evolved in our struggle to survive. Our minds have evolved to fit our circumstances, just as our bodies have.

In a different universe, supposing that life and consciousness could have existed, we would have evolved a different logic just as we would have evolved different bodies. I am not very happy with this point of view, but, until it is answered, I do not think we can conclude from the fact that we can understand the world that it had a designer. And I would like to point out that it is not perfectly certain yet that we can understand the world. To say that the world obeys the laws of logic is to say that neither the world nor any part of it can be the subject of two true propositions of the kind which logic asserts to be inconsistent with each other. Well, even if the world does not violate this law, the reader of modern atomic theory must sometimes feel that it is getting pretty near to doing so. When we are told, for example, that a light quantum is at one and the same time large enough to fill the object glass of a telescope and small enough to enter an atom, we may wonder what has happened to the law of contradiction. The apparent dilemma, we are told, points to a necessary revision of our notions of space and time. This means, I suppose, that a new mathematical web will be woven, and this supports the contention of the mathematical logicians that mathematics will always prove adequate to anything.

These remarks have been, perhaps, in the nature of a digression from my main subject. But before we can have clear ideas on the cultural value of mathematics we must know what it does for us, what its nature is. Is it a science? Is it a game? Is it an art? So far as we have gone it seems to be more like a game than anything else. And there is a school of Continental mathematicians which asserts that this is what it is. Mathematics proper, they assert, is a sort of game, played with meaningless marks on paper, rather like noughts and crosses. They assert that mathematical analysis, as ordinarily taught, cannot be regarded as a body of truth, but is either false or at best a meaningless game. Not all mathematical logicians agree with this. There are some who still think that mathematics can be reduced to formal logic. I know of nothing more difficult than these attempts to pry into the nature of mathematics. And it is distressing to find, when one has reached the point of exhaustion, that the conclusions are still subject to controversy. So I shall leave on one side these fundamental investigations. I will merely remark, incidentally, that if mathematics is a game like noughts and crosses, its cultural value is presumably low.

But, raising the subject to a more superficial level, I will ask

whether mathematics has not many of the characteristics of an art. In the first place I would point out that its history is not unlike that of an art, and particularly of the art of music. Like music, it seems to have an inherent power of development. We have all heard the remark that you only get out of a mathematical argument what you put into it, and it has always seemed to me that this remark is true only in a trivial sense. This remark was popularized by Thomas Henry Huxley, who was no mathematician, and who was engaged in a controversy at the time. I prefer a remark, that I came across a little while ago, by Heinrich Hertz. He says:

"One cannot escape the feeling that these mathematical formulae have an independent existence and an intelligence of their own, that they are wiser than we are, wiser even than their discoverers, that we get more out of them than was originally put into them."

The history of mathematics seems to bear out this assertion. Doubtless many of the primary mathematical concepts were suggested by experience, but many mathematical concepts have not been suggested by experience at all, but arose, quite without prevision, from the mathematical symbolism employed. Thus negative numbers were not suggested by experience. They arose automatically from the formula for subtraction, and they were so far from being foreseen that for a long time they were not understood. Even in the time of Newton, John Wallis, a very able mathematician, believed that negative numbers were greater than infinity. And for a long time the negative roots of equations were unintelligible to mathematicians. They simply neglected them. The mathematical machine had ground out these results, as it were, but they were so far from having been put into it that their emergence caused something like consternation. This is true, also, of imaginary numbers. When they first appeared no meaning could be attached to them. They were dismissed as meaningless products of the mathematical machine. There are innumerable instances of what we might call this self-generative power of mathematics. Thus mathematicians were led on to invent algebraic equations of higher and higher orders purely from considerations of abstract form. Each equation suggested the next. It was not that Nature was suggesting to them more and more complicated problems. The greater part of mathematics, in fact, has developed in accordance with an inner life of its own, although there are occasions, of course, where branches of analysis have arisen to meet the demands made by certain physical problems. On the whole, however, mathematics has been remarkably autonomous. In this respect it resembles the art of music more closely than any other art. Music is the least dependent on experience, on suggestions from the external world, of all the arts. Music owes practically nothing, for example, to the sounds that occur in nature. And the musician, except in the sort of music called programme music, is not concerned to describe or illustrate anything in external nature. Music, at least as much as mathematics, lives in a world of its own. Its development, also, has not been dissimilar. It has been a step by step development, not

because, like a science, it has been accommodating itself more and more closely to an external world, but because primitive musical forms suggest more complicated forms. We see this in the development of the scale, in the development of counterpoint, and in the development of harmony, as well as in the development of such a great musical form as the sonata form. In both mathematics and music their independence of experience and their purely formal development are distinguishing characteristics. If mathematics is an art, it is more like music than it is like any other art, and it may be suspected that it appeals to not dissimilar sensibilities. In this connection it is interesting to notice that many mathematicians have been musical. A statistical study would show, I think, that music is the favourite art of mathematicians. So far as I can judge, from a necessarily very imperfect knowledge, mathematicians are not often interested in painting, the art which is most dependent on the external world. Also, their literary taste, judging from the poetry they quote, is sometimes deplorable. But if they like music, they usually like good music. I have met only one mathematician, an artist in his mathematics, who was really indifferent to music. But he had a brother who was a very good musician.

Another point of resemblance between mathematics and music is the fact that they are the only two activities where we have the creative infant prodigy. The young Mozart and the young Pascal have not their like elsewhere. And this is what we should expect if Music and Mathematics are so largely independent of experience. The young mathematician and the young musician can proceed to create before they have learned anything about the world because, as a matter of fact, there is nothing for them to learn.

One of the chief functions of an art is to give aesthetic pleasure, and before we decide that the cultural value of mathematics is that of an art, we must ask whether mathematics gives aesthetic pleasure. I do not think there is much difficulty about maintaining this. We all know, as a matter of fact, that mathematics has a very strong aesthetic aspect. Every mathematician feels the difference between an " elegant " proof and a proof which, as Lord Rayleigh said, " merely commands assent ". Everyone realizes the difference between the mathematician who is an artist and the mathematician who is merely a workman. Many mathematicians have written about mathematics in a kind of prose poetry. Sylvester, who apparently saw all the colours of a sunset in a page of algebra, is a celebrated example, but I cannot forbear to quote a perhaps lesser known example from Boltzmann, quoted by Max Planck in the recent Maxwell Commemoration Volume. Boltzmann is describing a paper by Maxwell on the Kinetic Theory of Gases. He says :

" At first are developed majestically the Variations of the Velocities, then from one side enter the Equations of State, from the other the Equations of Motion in a Central Field ; ever higher sweeps the chaos of Formulae ; suddenly are heard the four words : " put $n=5$ ". The evil spirit V (the relative velocity of two molecules) vanishes and the dominating figure in the bass is suddenly silent ;

that which had seemed insuperable being overcome as if by a magic stroke. There is no time to say why this or that substitution was made ; who cannot sense this should lay the book aside, for Maxwell is no writer of programme music, who is obliged to set the explanation over the score. Result after result is given by the pliant formulae till, as unexpected climax, comes the Heat Equilibrium of a heavy gas ; the curtain then drops."

We must admit, I think, that, whatever we may think of Boltzmann's analogy, he is certainly expressing a strong aesthetic reaction. And it was Henri Poincaré, I think, who is reported to have said that in all his researches he had never been interested to find the value of x, but solely in the beauty of the methods by which he found that value. There is no need to multiply examples. Every mathematician knows that one of the chief charms, perhaps the chief charm, of mathematics, resides in its aesthetic aspect.

I conclude, therefore, that the cultural value of mathematics is that of an art, and that the art it most closely resembles is the art of music. The cultural value of music is to be found, I think, in the states of consciousness it awakes in us. A great piece of music makes us aware of feelings, of sensibilities, that perhaps we had never experienced before. A thousand shades of joy and sorrow, a refinement of ecstasy, a purity of desire, such as we have never yet known, can be evoked in us by music. Our inner life is enriched, and that is the only life worth enriching. And I claim for mathematics that it does a similar thing for the more intellectual part of our mind. It subtilizes and refines our intellectual capacities as music subtilizes and refines our emotions. The student of mathematics passes continually from one level of abstraction to another. Such notions as the general concept of number, of a function, of continuity, really are additions to the resources of the mind. The student finds that ideas which were at first too elusive to be grasped, gradually become permanent additions to his mental furniture. The man who lacks these resources, to whom these ideas are meaningless, does not realize the possibilities of the mind. He lives in a poorer world. He lacks certain sensibilities. To that extent he is uncultured. And if modern science persists with its present tendency of reducing the universe to mathematical relations between entities which are unimaginable, and can only be mathematically defined, of making the universe, in fact, a completely mathematical affair, then the non-mathematician will be completely at a loss. A fundamental department of knowledge will become completely inaccessible to him. He will be, in one very important respect, uncultured. Mathematics will then acquire a new cultural status. It will be valued for the part it plays in refining and educating certain mental sensibilities, and also for the part it plays in providing the key to information which is indispensable to a truly cultured outlook. But even if it turns out that the universe is not essentially mathematical, that God is not, after all, a pure mathematician, it can still be maintained, I think, that mathematics is an essential part of culture.

J. W. N. SULLIVAN.

AVOIDING SYMBOLIC METHODS IN THE SOLUTION OF DIFFERENTIAL EQUATIONS.

By R. J. A. Barnard.

The discussion on symbolic operators in differential equations in recent numbers of the *Gazette* shows the need of a different method of attacking the linear equation of the second order with constant coefficients, which is the case most wanted in practice. It seems a pity that no method is given in the English books except the symbolic one, in which pass students get lost owing to their not understanding exactly what they can or cannot do with the operators. And is this to be wondered at when a theorem which gives erroneous results if not interpreted in a particular way has stood in Forsyth's *Treatise* * for forty years ?

For some years I have been trying various methods of dealing with these equations, with the object of avoiding the symbolic methods. I give here in outline the two principal methods which I have used in my pass classes at Melbourne University.

1. The first was suggested to me by Professor Michell, who wished to avoid not only symbolic methods, but also the use of complex numbers. Briefly, it was the method of removing the middle term, then using integrating factors to get two first integrals from which the first derivative was then eliminated. It ran as follows, successive steps being lettered (*a*), (*b*), (*c*) ... .

(*a*) Discussion of the equations

$$d^2y/dx^2 - n^2y = 0, \quad \ldots\ldots(1)$$
$$d^2y/dx^2 + n^2y = 0, \quad \ldots\ldots(2)$$
$$d^2y/dx^2 = 0, \quad \ldots\ldots(3)$$

showing how the complete solutions

(1) $A \sinh nx + B \cosh nx$

or $Ae^{nx} + Be^{-nx}$,

(2) $A \sin nx + B \cos nx$,

(3) $A + Bx$,

can be obtained by multiplying, in (1) and (2), by dy/dx and integrating, then extracting the square root and integrating again.

(*b*) The proof that any solution of

$$d^2y/dx^2 \pm n^2y = 0$$

is an integrating factor of

$$d^2y/dx^2 \pm n^2y = X(x).$$

(*c*) The solution of

$$d^2y/dx^2 - n^2y = X$$

by multiplication in turn by $\cosh nx$ and $\sinh nx$, or by e^{nx} and e^{-nx}, giving on integration

* *Treatise on Differential Equations*, 5th ed., p. 78, iv.

$$\cosh nx \,.\, \frac{dy}{dx} - ny \sinh nx = A + \int X \cosh nx \, dx,$$

$$\sinh nx \,.\, \frac{dy}{dx} - ny \cosh nx = B + \int X \sinh nx \, dx,$$

the elimination of dy/dx giving the result. A similar method applies to

$$d^2y/dx^2 + n^2y = X,$$

and for uniformity of treatment, and to avoid iterated integrals in the general solution, the equation

$$d^2y/dx^2 = X$$

was treated in the same way using integrating factors 1 and x.

(*d*) The general equation

$$d^2y/dx^2 + a\, dy/dx + by = X$$

was reduced to the previous form by the substitution

$$y = z \exp(-\tfrac{1}{2}ax),$$

and hence the complete solution was obtained.

(*e*) Having found the complete solution in all cases, the terms Complementary Function and Particular Integral could be explained and the former found from the auxiliary equation. Examples then showed the form of solution to be expected from the commoner forms of X, and the consequent use of tentative methods was insisted on, as the shortest method of obtaining a particular solution. When the tentative method failed, recourse could always be had to the general method, which in fact in these cases is simpler than in the more general cases.

2. As this method was rather long, I have adopted a quite different method in the last two years, which still avoids symbolic methods but introduces complex numbers, as far as using

$$\exp(ix) = \cos x + i \sin x.$$

I use the principle that the equation

$$d^2y/dx^2 + a\, dy/dx + by = X \quad \ldots\ldots\ldots\ldots\ldots\ldots (4)$$

can be reduced to an equation of the first order if a solution of

$$d^2y/dx^2 + a\, dy/dx + by = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots (5)$$

is known. The steps in this method are :

(*a*) real solutions of (5) are found by the ordinary method of the auxiliary equation. Thus we have solutions of the form

$\exp(-\tfrac{1}{2}ax)\cosh nx$, $\exp(-\tfrac{1}{2}ax)\sinh nx$, if $a^2 > 4b$;
$\exp(-\tfrac{1}{2}ax)\cos nx$, $\exp(-\tfrac{1}{2}ax)\sin nx$, if $a^2 < 4b$;
$\exp(-\tfrac{1}{2}ax)$, if $a^2 = 4b$:

(*b*) if u is such a solution, putting $y = uz$ we get

$$\frac{d^2z}{dx^2} + \left(a + \frac{2}{u}\frac{du}{dx}\right)\frac{dz}{dx} = \frac{X}{u};$$

(*c*) this equation, being linear and of the first order in dz/dx, is solved by using the integrating factor $u^2 \exp(ax)$, and gives

$$u^2 \exp(ax) \cdot \frac{dz}{dx} = A + \int Xu \exp(ax)\, dx,$$

or $$\frac{dz}{dx} = Au^{-2} \exp(-ax) + u^{-2} \exp(-ax) \int Xu \exp(ax)\, dx: \quad \ldots(6)$$

(*d*) taking the more difficult case $a^2 < 4b$, write

$$u = \exp(-\tfrac{1}{2}ax) \cos nx, \quad n^2 = b - \tfrac{1}{4}a^2,$$
$$u^2 \exp(ax) = \cos^2 nx,$$

and then $$\frac{dz}{dx} = A \sec^2 nx + \sec^2 nx \int X \exp(\tfrac{1}{2}ax) \cos nx\, dx;$$

the second term may be integrated by parts and we have

$$z = \frac{A}{n} \tan nx + B + \frac{1}{n} \tan nx \int X \exp(\tfrac{1}{2}ax) \cos nx\, dx$$
$$- \frac{1}{n} \int X \exp(\tfrac{1}{2}ax) \sin nx\, dx,$$

$$y = \exp(-\tfrac{1}{2}ax) \left[\frac{1}{n} \sin nx \left\{ A + \int X \exp(\tfrac{1}{2}ax) \cos nx\, dx \right\} \right.$$
$$\left. + \cos nx \left\{ B - \frac{1}{n} \int X \exp(\tfrac{1}{2}ax) \sin nx\, dx \right\} \right],$$

the same form as obtained by the first method, but more shortly. Tentative methods are insisted on as before; they fail when X is a solution of the homogeneous equation (5), but the integrations are then much simpler, as we can see from the preceding work by taking $X = \exp(-\tfrac{1}{2}ax) \cos nx$. The student may use the tentative methods in all other cases, and use this general method in the cases of failure only.

It may be noticed that the integrations can always be carried out if X is of the form

$$\Sigma x^m \exp(rx) \sin(sx + \alpha) \ \ldots \ (m \text{ a positive integer}).$$

If X is a polynomial, a particular solution is a polynomial of the same degree in general, the case of failure being $b = 0$, which is a specially simple case giving a polynomial of degree higher by one.

3. Another method, starting as before from a solution of the homogeneous equation (5), is as follows. If u is such a solution,

$$d^2u/dx^2 + a\, du/dx + bu = 0. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(7)$$

Eliminate the term in y from (4) and (7), giving

$$u\, d^2y/dx^2 - y\, d^2u/dx^2 + a\,(u\, dy/dx - y\, du/dx) = Xu,$$

a linear equation in $u \dfrac{dy}{dx} - y \dfrac{du}{dx}$, whence

$$u \frac{dy}{dx} - y \frac{du}{dx} = A \exp(-ax) + \exp(-ax) \int Xu \exp(ax\, dx)$$

or $$\frac{d}{dx}\left(\frac{y}{u}\right) = Au^{-2} \exp(-ax) + u^{-2} \exp(-ax) \int Xu \exp(ax)\, dx,$$

as in (6), and the work proceeds as before.

R. J. A. B.

ON A CONTINUED FRACTION OF STIELTJES.

By E. G. Phillips.

1. One of the two curious self-reciprocal formulae stated by Ramanujan,*

$$\int_0^\infty g(x)\sin\tfrac{1}{2}\pi nx\,dx = g(n) \qquad (1.1)$$

where
$$g(x) = \frac{1}{x+}\,\frac{1^2}{x+}\,\frac{2^2}{x+}\,\frac{3^2}{x+}\cdots, \qquad (1.2)$$

of which I have recently given a proof,† can be established by using a result proved by Stieltjes ‡ that, if $x>0$, $m\geqslant 0$, $0\leq a\leq 1$,

$$\int_0^\infty \frac{e^{-xu}\,du}{(\cosh u + a\sinh u)^m}$$
$$= \frac{1}{x+ma+}\,\frac{m(1-a^2)}{x+(m+2)a+}\,\frac{2(m+1)(1-a^2)}{x+(m+4)a+}\,\frac{3(m+2)(1-a^2)}{x+(m+6)a+}\cdots. \qquad (1.3)$$

The proof only requires the above result in the case in which $m=1$, $a=0$. While seeking an extension of the result (1. 1), I have obtained an expression for the continued fraction (1. 3) in the form of an infinite series.

Although it is almost certain that no extension of Ramanujan's formula (1. 1) can be found in a form which involves continued fractions, an extension of a kind can be made by using a theorem proved by Hardy and Titchmarsh § that if $f(x)$ is its own J_μ transform, then

$$h(x) = \int_0^\infty f(u)\,K_{\frac{1}{2}(\nu-\mu)}(xu)\,(xu)^{\frac{1}{2}(\nu+\mu+1)}\,du$$

is its own J_ν transform. If $\mu=-\frac{1}{2}$ and $f(x)=\operatorname{sech} x\sqrt{(\frac{1}{2}\pi)}$, then

$$h(x) = \int_0^\infty \operatorname{sech} u\sqrt{(\tfrac{1}{2}\pi)}\,K_{\frac{\nu}{2}+\frac{1}{4}}(xu)\,(xu)^{\frac{\nu}{2}+\frac{1}{4}}\,du$$

is its own J_ν transform, that is

$$h(x) = \int_0^\infty (xy)^{\frac{1}{2}} J_\nu(xy)\,h(y)\,dy.$$

When $\nu=\frac{1}{2}$ this reduces to Ramanujan's result (1. 1).

2. Consider the integral on the left of (1. 3), first for the case when $a=0$. By writing $e^{-2u}=t$ the integral

$$\int_0^\infty \frac{e^{-xu}\,du}{\cosh^m u} \qquad (2.1)$$

becomes
$$2^{m-1}\int_0^1 \frac{t^y\,dt}{(1+t)^m}, \qquad (2.2)$$

where $y=\frac{1}{2}(x+m)-1$.

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* *Collected Papers*, 334.

† *Journal London Math. Soc.* 4 (1929), 310-13.

‡ *Quart. J. of Math.* 35 (1904), 195.

§ *Proc. London Math. Soc.* (2), 33 (1931), 226.

Now
$$\int_0^1 t^y(1+t)^{-m}\,dt$$
$$=\frac{1}{y+1}-\frac{m}{y+2}+\frac{m(m+1)}{2!}\frac{1}{y+3}-\dots$$
$$+(-)^{n+1}\frac{m(m+1)\dots(m+n-2)}{(n-1)!}\frac{1}{y+n}+\dots, \qquad (2.3)$$

the integration up to $t=1$ being valid if the series is convergent. Now for the series (2. 3)

$$\frac{u_n}{u_{n+1}}=1+\frac{2-m}{n}+O\left(\frac{1}{n^2}\right), \qquad (2.4)$$

and so the series (2. 3) is absolutely convergent if $m<1$ and the series of moduli is divergent if $m\geqslant 1$. There may be conditional convergence of the series (2. 3) however, and by the alternating series test we see that $u_{n+1}\leq u_n$ provided that $n\geqslant (m-1)y/(2-m)$ and $u_n\to 0$ as $n\to\infty$, if $m<2$.

Hence the series (2. 3) converges conditionally if $1\leq m<2$.

Since the continued fraction (1. 3) when $a=0$ converges if $x>0$, $m\geqslant 0$, the series (2. 3) multiplied by 2^{m-1} is equivalent to the continued fraction

$$\frac{1}{x+}\,\frac{m}{x+}\,\frac{2(m+1)}{x+}\,\frac{3(m+2)}{x+\dots} \qquad (2.5)$$

if $x>0$, $0\leq m<2$.

3. Consider the integral on the left of (1. 3) when $a\neq 0$.

By writing $e^{-2u}=t$ the integral becomes

$$\frac{2^{m-1}}{(1+a)^m}\int_0^1 \frac{t^y\,dt}{(1+\lambda t)^m}. \qquad (3.1)$$

where $\lambda=(1-a)/(1+a)$. If $\lambda<1$ we see that the series

$$\frac{2^{m-1}}{(1+a)^m}\left\{\frac{1}{y+1}-\frac{m\lambda}{y+2}+\frac{m(m+1)}{2!}\frac{\lambda^2}{y+3}-\dots\right.$$
$$\left.+(-)^{n+1}\frac{m(m+1)\dots(m-n+2)}{(n-1)!}\frac{\lambda^{n-1}}{y+n}+\dots\right\} \qquad (3.2)$$

is absolutely convergent and is equivalent to the continued fraction (1. 3). Since the continued fraction only converges when $0\leq a\leq 1$, the only range of values of λ under consideration is $0\leq\lambda\leq 1$. The case $\lambda=1$ is the same as $a=0$ which is already considered in § 2. It follows that Stieltjes' continued fraction (1. 3) is equivalent to the infinite series (3. 2) if $x>0$, $m\geqslant 0$, $0<a\leq 1$.

Since the integrals (2. 2) and (3. 1) can only be expressed in terms of elementary functions if either y, m or $y-m$ is an integer, the value of the continued fraction or the sum of the infinite series may be expressed in terms of elementary functions under the same restrictions on y and m. E. G. P.

A NEW SEQUENCE IN THE TEACHING OF TRIGONOMETRY.

By H. Abson.

One of the chief appeals of Trigonometry to the beginner lies in the practical nature of the subject. The first aim in this respect is to obtain the formulae for the solution of the general triangle, but hitherto there has been much delay in the logical development of the subject up to that stage because of the need

(*a*) to prove the half-angle formula $\cos A = \cos^2 \frac{1}{2}A - \sin^2 \frac{1}{2}A$ and

(*b*) to show that $\cos (180° - A) = -\cos A$ and

$$\sin (180° - A) = \sin A.$$

This delay, generally combined with much seemingly irrelevant discussion of the ratios of angles of any magnitude and the proof of the ratios of compound angles in order to obtain the half-angle formulae, has tended to dissipate the learner's interest in the subject, encouraged in the beginning by its practical nature, evident in the use of right-angled triangles.

The following sequence avoids these objections to the usual method of treatment and leads straight to the formulae for the solution of the general triangle, both with and without logarithms.

1. Definition of ratios for acute angles with emphasis on the complementary relations $\cos (90° - A) = \sin A$, etc., and the proof of $\sin^2 A + \cos^2 A = 1$ as a deduction from Pythagoras' Theorem, which, incidentally, is easily proved in terms of Trigonometry.

2. In discussing the merits of the calculation of ratios as against their determination by ruler and protractor, the formula

$$\cos A = \cos^2 \tfrac{1}{2}A - \sin^2 \tfrac{1}{2}A = 2 \cos^2 \tfrac{1}{2}A - 1 = 1 - 2 \sin^2 \tfrac{1}{2}A,$$

which is easily proved geometrically for acute angles, is seen to be of use in calculating other ratios.

3. Then, on the assumption that the ratios of obtuse angles will satisfy the same relations as those of acute angles, we have

$$\begin{aligned} \cos (180° - A) &= \cos^2 (90° - \tfrac{1}{2}A) - \sin^2 (90° - \tfrac{1}{2}A) \\ &= \sin^2 \tfrac{1}{2}A - \cos^2 \tfrac{1}{2}A \\ &= -\cos A \end{aligned}$$

and

$$\begin{aligned} \sin (180° - A) &= 2 \sin (90° - \tfrac{1}{2}A) \,.\, \cos (90° - \tfrac{1}{2}A) \\ &= 2 \cos \tfrac{1}{2}A \,.\, \sin \tfrac{1}{2}A \\ &= \sin A. \end{aligned}$$

4. With these results

$$a^2 = b^2 + c^2 - 2bc \cos A$$

and

$$a/\sin A = b/\sin B = c/\sin C$$

can be proved for any triangle. Then $\cos A = 2 \cos^2 \frac{1}{2}A - 1$ is used to prove that

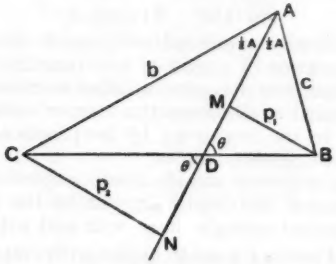
$$\cos \tfrac{1}{2}A = \sqrt{\frac{s(s-a)}{bc}}, \text{ etc.}$$

5. The new geometrical proof of the $\tan \frac{1}{2}(B-C)$ formula given below completes the formulae needed for the logarithmic solution of the general triangle.

6. Later on, the assumption that the ratios of any angle must be defined so as to satisfy the acute-angle formulae, provides a basis for the extended definitions and shows clearly why the radius vector should be kept positive. This appeals to the beginner much more than the arbitrary re-defining of the ratios for angles of any size.

To prove that in any triangle

$$\tan \tfrac{1}{2}(B-C) = \frac{b-c}{b+c} \cot \tfrac{1}{2}A.$$



Suppose $b > c$. Let AD bisect $\angle A$ and meet BC at D, and let BM and CN, (p_1 and p_2), be the perpendiculars from B and C on to AD. Let $\angle ADB = \theta$.

$$MN = MD + DN = p_1 \cot \theta + p_2 \cot \theta.$$

But $\quad p_1 = c \sin \frac{1}{2}A \quad$ and $\quad p_2 = b \sin \frac{1}{2}A$;

thus $\quad MN = (b+c) \cot \theta \,.\, \sin \frac{1}{2}A.$

Also $\quad MN = AN - AM = b \cos \frac{1}{2}A - c \cos \frac{1}{2}A$

$$= (b-c) \cos \tfrac{1}{2}A ;$$

$$(b+c) \cot \theta \,.\, \sin \tfrac{1}{2}A = (b-c) \cos \tfrac{1}{2}A ;$$

$$\cot \theta = \frac{b-c}{b+c} \cot \tfrac{1}{2}A.$$

Finally, $\quad \theta = C + \frac{1}{2}A = 90° - \frac{1}{2}(B-C)$;

and $\quad \cot \theta = \tan \frac{1}{2}(B-C)$,

and the result follows. H. A.

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873. We are two parallel lines drawn very close to each other. . . . So close indeed that no third line, however thin, could be drawn between them.

Will the two parallel lines ever meet ?

Yes. In infinity.

Ali Hasan !, I exclaimed, had you ever dreamed that there was so much poetry and pathos and sorrow in mathematics ?—G. S. Viereck and P. Eldridge, *My First Two Thousand Years*, p. 205 (London, 1929). [Per Mr. Frank Robbins.]

MATHEMATICAL NOTES.

1037. *The Derivative of x^n.*

The proof for the derivative of x^n by assuming the binomial series for a rational index, which is given in some text-books, may be completed as follows:

If $x \neq 0$, and $|h| < |x|$ and $\left|\frac{x}{n}\right|$,

$$\frac{(x+h)^n - x^n}{h} = \frac{x^n}{h}\left\{\left(1+\frac{h}{x}\right)^n - 1\right\},$$

$$= nx^{n-1} + x^{n-1}\left\{\frac{n(n-1)}{1.2}\frac{h}{x} + \frac{n(n-1)(n-2)}{1.2.3}\frac{h^2}{x^2} + \ldots\right\},$$

$$= nx^{n-1} + x^{n-1}R(x, h).$$

If $|n| < 1$, then $|n-1| < 2$, $|n-2| < 3$, ... and we have

$$|R(x, h)| < \left|\frac{h}{x}\right| + \left|\frac{h}{x}\right|^2 + \ldots,$$

$$< \frac{|h|}{|x| - |h|}.$$

If $|n| \geqq 1$, then $|n-1| \leqq 2|n|$, $|n-2| \leqq 3|n|$, and we have

$$|R(x, h)| \leqq |n|\left\{\left|\frac{nh}{x}\right| + \left|\frac{nh}{x}\right|^2 + \ldots\right\},$$

$$\leqq \frac{|n|^2|h|}{|x| - |nh|}.$$

In either case $\lim_{h\to 0} R(x, h) = 0$, and hence

$$\lim_{h\to 0} \frac{(x+h)^n - x^n}{h} = nx^{n-1}.$$

H. V. MALLISON.

1038. *The Logarithmic Scale.*

In dealing with the convergence of series of positive terms, the theorem is required that $\log n$ tends to infinity more slowly than any positive power of n, in order to obtain the logarithmic scale of convergence. This result may be obtained from Euler's limit, a proof of which is given depending on series of positive terms:

$$-\frac{1}{n} - \log\left(1 - \frac{1}{n}\right) = \frac{1}{2n^2} + \frac{1}{3n^3} + \ldots, \quad (n > 1)$$

$$< \frac{1}{2n^2}\left(1 + \frac{1}{n} + \frac{1}{n^2} + \ldots\right),$$

$$< \frac{1}{2n^2}\frac{n}{n-1} < \frac{1}{n^2}.$$

Thus, as $n \to \infty$, $\sum_2^n \left\{ -\frac{1}{n} - \log\left(1 - \frac{1}{n}\right) \right\}$ tends to a limit which is positive and less than

$$\sum_2^n \frac{1}{n^2} < \left(\frac{1}{1.2} + \frac{1}{2.3} + \ldots\right) < 1.$$

Subtracting the expression from 1 and expanding the sum, we have $1 + \frac{1}{2} + \frac{1}{3} + \ldots + \frac{1}{n} - \log n$ tends to a limit γ as n tends to infinity.

Consider now $\left(1 + \frac{1}{2} + \frac{1}{3} + \ldots + \frac{1}{n}\right) n^{-\frac{1}{p}}$, where p is any positive integer.

Let m be an integer less than n to be determined, r the quotient of n by m (*i.e.* the greatest integer in n/m).

Then

$$\left(1 + \frac{1}{2} + \frac{1}{3} + \ldots + \frac{1}{n}\right) n^{-\frac{1}{p}} < \left(m + m.\frac{1}{m} + m.\frac{1}{2m} + \ldots + m.\frac{1}{rm}\right) n^{-\frac{1}{p}},$$

(by considering each group of m terms, the last group having possibly less than m terms)

$$< mn^{-\frac{1}{p}} + \left(1 + \frac{1}{2} + \ldots + \frac{1}{r}\right) n^{-\frac{1}{p}}.$$

Again, if s is the quotient of r by m,

$$\left(1 + \frac{1}{2} + \frac{1}{3} + \ldots + \frac{1}{n}\right) n^{-\frac{1}{p}} < 2mn^{-\frac{1}{p}} + \left(1 + \frac{1}{2} + \ldots + \frac{1}{s}\right) n^{-\frac{1}{p}};$$

and finally $$< kmn^{-\frac{1}{p}},$$

where $m^k < n < m^{k+1}$, k being a positive integer.

Choose m to be the greatest integer in $n^{\frac{1}{p+1}}$.

We then have

$$\left(1 + \frac{1}{2} + \frac{1}{3} + \ldots + \frac{1}{n}\right) n^{-\frac{1}{p}} < kn^{-1/p(p+1)},$$

which $\to 0$ as $n \to \infty$.

Hence $$\lim_{n \to \infty} (\log n)/n^{\frac{1}{p}} = 0.$$

It follows from the multiplication of limits that

$$\lim_{n \to \infty} (\log n)^{\Delta}/n^{\delta} = 0,$$

where Δ, δ are any positive rational numbers. H. V. MALLISON.

1039. *Accuracy of Figures. A comment on a paragraph in "The Primary School" (Hadow Report).*

For some years "Contracted Methods" in arithmetic have been

falling into disfavour. Inspectors and representatives of the Board of Education at Summer Courses have encouraged a suitable discussion of the accuracy of figures as an alternative. Such alternatives have been developed in many schools and are appearing in print in the latest books. It is therefore surprising that a Board of Education Report of such authority as *The Primary School* should contain the following—p. 181, lines 24-29 :

" If the data are only true to the first decimal place, the calculation should go no farther than that place ; all figures in the answer subsequent to the first decimal place are meaningless, and to allow them is to blur the child's knowledge of his limitations in powers of measurement."

Actually the first decimal place of an answer if the data are as above may be just as meaningless as the other decimal places. In fact, no satisfactory rule can be stated in terms of " decimal places ". " Significant figures " must be used, and at this stage in arithmetic they have become more important than " decimal places ".

A suitable discussion of the accuracy of figures may be divided into two parts :

(*a*) discovering which figures of an answer are useless after working with numbers whose last figure is only approximate ;

(*b*) discovering if any figures may be discarded before an operation with numbers of several figures.

The following two satisfactory rules, for multiplication and division, are easily discovered by working out the maximum and minimum products and quotients of two approximate numbers when the maximum error in each is allowed for.

Rule (*a*). An answer to an operation with two numbers should be given correct to as many significant figures as the less accurate of the two numbers. Even then the last figure of the answer, though useful, may not be exact.

Rule (*b*). For each operation with two numbers work with one more significant figure in each than is required in the answer.

Rule (*a*) is the one which should replace the lines quoted from the Hadow Report. Rule (*b*) follows directly from Rule (*a*) and replaces " Contracted Methods " for school purposes. It allows a great amount of interesting work on statistics, rates and other subjects to be done simply.

H. BERRY.

1040. *Note on a type of Matrix Product.*

Such a matrix product as

$$\begin{bmatrix} 2 & . & . & . \\ 3 & 2 & . & . \\ . & 3 & 2 & . \\ . & . & 3 & 2 \end{bmatrix} \begin{bmatrix} 3 & . & . & . \\ 2 & 3 & . & . \\ 4 & 2 & 3 & . \\ . & 4 & 2 & 3 \end{bmatrix} = \begin{bmatrix} 6 & . & . & . \\ 13 & 6 & . & . \\ 14 & 13 & 6 & . \\ 12 & 14 & 13 & 6 \end{bmatrix}$$

may be set down and worked after the manner of ordinary multiplication. The corresponding matrix quotient may also be found by ordinary division, thus :

	4	2	3		32)	(12)	(14)	(13)	6 (	423
		3	2			(12)	8			
	8	4	6			.	6	(13)		
(12)	6	9	.				6	4		
(12)	(14)	(13)	6				.	9	6	
								9	6	
								.	.	

The commutative rule holds for both multiplication and division of this type of matrix.

The reason is that if J is a matrix consisting of units in the first lower diagonal with zeros elsewhere, then J^2 consists of units in the second lower diagonal with zeros elsewhere, etc.

The justification of the method is then completed as follows :

$$\begin{array}{r} 4J^2 + 2J + 3I \\ 3J + 2I \\ \hline 12J^3 + 6J^2 + 9J \\ \quad 8J^2 + 4J + 6 \\ \hline 12J^3 + 14J^2 + 13J + 6 \end{array}$$

In fact, we may use J as a radix in building up this type of matrix just as we use 10 as a radix in building up ordinary real numbers. Some of the consequences of doing this I have shown elsewhere.

V. NAYLOR.

1041. *A proof of the theorems of Feuerbach and Hart.*

This proof of Feuerbach's theorem seems to have escaped notice. It depends on the device of transforming the inscribed circle and one escribed circle into concentric circles by the method of inversion.

Take two circles, radii a and b, respectively, with common centre O. Call them 1 and 2. Take also two other circles 3 and 4 with the same centre O, and radii $\frac{1}{2}(a+b)$ and $\frac{1}{2}(a-b)$, respectively. Then there are two classes of circles which touch 1 and 2, namely :

(i) circles with centre on 3 and radius $\frac{1}{2}(a-b)$;

(ii) circles with centre on 4 and radius $\frac{1}{2}(a+b)$.

Take two circles with centres A and B of class (i) and two circles with centres C and X of class (ii), and suppose that the chords AB, CX are equal. Then, denoting these circles by the same letters as their centres, the angle of intersection of the circles A and C, being the angle of a triangle of sides AC, $\frac{1}{2}(a+b)$, $\frac{1}{2}(a-b)$ is equal to the angle AOC, whilst the angle of intersection of the circles A and B is equal to COX, since CX, AB are equal. If, then, we denote the angles of intersection of the circles A, B, C by A', B', C', we have

found two circles X_1 and X_2 such that the angles of intersection of

X_1 with A, B, C are $B'-C',\ C'-A',\ A'-B'$;

X_2 with A, B, C are $B'+C',\ C'+A',\ A'-B'$,

and X_1 and X_2 are reflections of each other in OC.

Now transform the figure by inversion from any point, using the same letters as before, and regard A, B, C and 1 as given, then the circle X which is sufficiently determined as a circle touching 1 and cutting A, B, C at given angles $B'-C',\ C'-A',\ A'-B'$, will also touch 2; and therefore also the other two circles touching the circles A, B, C, which are obtained by interchanging C with A or B.

This is Hart's theorem.

In the figure corresponding to Feuerbach's theorem, the three circles A, B, C have a common point L (the centre of inversion), $A'+B'+C'=\pi$, and the circles A and B are reflections of one another in OL. Therefore X_2 is the reflection of C in OL, and X_2 passes through L, whilst X_1 will meet C in the point which is the reflection of L in the straight line OC; or, in other words, in the harmonic conjugate of L on the circle C with respect to the points of contact of C with 1 and 2.

Thus in the inverse figure the circle touching the inscribed circle, and making angles $B-C,\ C-A,\ A-B$ with the sides, also passes through the middle point of AB; whilst the inverse of X_2 is the fourth common tangent to the inscribed and escribed circles.

A. L. Dixon.

1042. *Solution of a system of equations.*

Consider the system of equations (a, b, c being unequal):

$$x^2+2yz=a, \quad \ldots\ldots(1)$$
$$y^2+2zx=b, \quad \ldots\ldots(2)$$
$$z^2+2xy=c. \quad \ldots\ldots(3)$$

Writing

$$a+b+c=p^2,$$
$$a+b\omega+c\omega^2=q^2,$$
$$a+b\omega^2+c\omega=r^2,$$

where $1+\omega+\omega^2=0$, we have

$$3a=p^2+q^2+r^2, \quad \ldots\ldots(4)$$
$$3b=p^2+q^2\omega^2+r^2\omega, \quad \ldots\ldots(5)$$
$$3c=p^2+q^2\omega+r^2\omega^2. \quad \ldots\ldots(6)$$

But the set (1)-(3) gives by adding,

$$x+y+z=\pm p. \quad \ldots\ldots(7)$$

Now choose λ, μ, ν, so that

(a) $$a\lambda+b\mu+c\nu=0, \quad \ldots\ldots(8)$$

and (b), Φ breaks up into linear factors, where we have written

$$\Phi\equiv\lambda x^2+\mu y^2+\nu z^2+2\lambda yz+2\mu zx+2\nu xy. \quad \ldots\ldots(9)$$

The condition for (b) is

$$\begin{vmatrix} \lambda, & \nu, & \mu \\ \nu, & \mu, & \lambda \\ \mu, & \lambda, & \nu \end{vmatrix}=0,$$

which we can write as

$$\lambda+\mu+\nu=0, \quad \ldots\ldots(9{\cdot}1)$$

or $$\lambda+\mu\omega+\nu\omega^2=0, \quad \ldots\ldots(9{\cdot}2)$$

or $$\lambda+\mu\omega^2+\nu\omega=0. \quad \ldots\ldots(9{\cdot}3)$$

Solving (8) with each part of (9) in turn, we have

$$\lambda:\mu:\nu=(c-b):(a-c):(b-a), \quad \ldots\ldots(10{\cdot}1)$$

$$\lambda:\mu:\nu=(c\omega-b\omega^2):(a\omega^2-c):(b-a\omega), \quad \ldots\ldots(10{\cdot}2)$$

$$\lambda:\mu:\nu=(c\omega^2-b\omega):(a\omega-c):(b-a\omega^2), \quad \ldots\ldots(10{\cdot}3)$$

and substituting from (4), (5) and (6) reduces these to

$$\lambda:\mu:\nu=(q^2-r^2):(\omega q^2-\omega^2r^2):(\omega^2q^2-\omega r^2), \quad \ldots\ldots(11{\cdot}1)$$

$$\lambda:\mu:\nu=(p^2-q^2):(\omega^2p^2-q^2):(\omega p^2-q^2), \quad \ldots\ldots(11{\cdot}2)$$

$$\lambda:\mu:\nu=(r^2-p^2):(r^2-\omega^2p^2):(r^2-\omega p^2). \quad \ldots\ldots(11{\cdot}3)$$

Any of these values must make Φ break up into linear factors, and must also make Φ vanish, by (8), since

$$a\lambda+b\mu+c\nu\equiv\Phi.$$

Substituting in $\Phi=0$ and factorising, either by inspection or by the general method, we have

$$(q+r)\,x+(\omega q+\omega^2r)\,y+(\omega^2q+\omega r)\,z=0, \quad \ldots\ldots(12{\cdot}1)$$

or $$(q-r)\,x+(\omega q-\omega^2r)\,y+(\omega^2q-\omega r)\,z=0\,; \quad \ldots\ldots(12{\cdot}2)$$

$$(p+q)\,x+(\omega^2p+q)\,y+(\omega p+q)\,z=0, \quad \ldots\ldots(13{\cdot}1)$$

or $$(p-q)\,x+(\omega^2p-q)\,y+(\omega p-q)\,z=0\,; \quad \ldots\ldots(13{\cdot}2)$$

$$(r+p)\,x+(r+\omega p)\,y+(r+\omega^2p)\,z=0, \quad \ldots\ldots(14{\cdot}1)$$

or $$(r-p)\,x+(r-\omega p)\,y+(r-\omega^2p)\,z=0. \quad \ldots\ldots(14{\cdot}2)$$

Combining pairs of these from different sets, and using (7) to give the value of each fraction, we have :

from (12·1), (14·1) ; (14·1), (13·2) ; (13·2), (12·1) ;

$$x:y:z:1=p+q-r:p+\omega q-\omega^2r:p+\omega^2q-\omega r:\pm3\,;$$

and three similar proportions, so that finally

$$x:y:z:1=(\pm p\pm q\pm r):(\pm p\pm\omega q\pm\omega^2r):(\pm p\pm\omega^2q\pm\omega r):3.$$

F. J. D. Syer.

1043. *Proof of the formula for the area of a cyclic quadrilateral.*

Let $ABCD$ be the quadrilateral, and let EMF be the perpendicular bisector of AC. Then DF, BF will bisect the angles ADC, CBA. Let EX, EY be perpendicular to AB, BC ; BG, DH perpendicular to EF ; BK, DJ perpendicular to AC, and FZ perpendicular to CD. Then

$$2BX=AB+BC,\ 2AX=AB-BC,\ 2DZ=AD+DC,$$
$$2CZ=AD-DC, \quad \ldots\ldots(1)$$

$$\text{Area } ABCD=MC\,.\,BK+MC\,.\,DJ$$
$$=MC\,.\,GH$$
$$=\sqrt{GH^2\,.\,EM}\,.\,MF, \text{ since } EM\,.\,MF=MC^2,$$
$$=\sqrt{\{(FH\,.\,EM-FG\,.\,EM)(GE\,.\,FM-EH\,.\,FM)\}} \ldots(2)$$

Now since $BGXE$, $EMXA$ are cyclic,

$$\alpha=\beta=\gamma=\delta=\theta.$$

Hence $EM \cdot EG = EX^2$, ...(3)

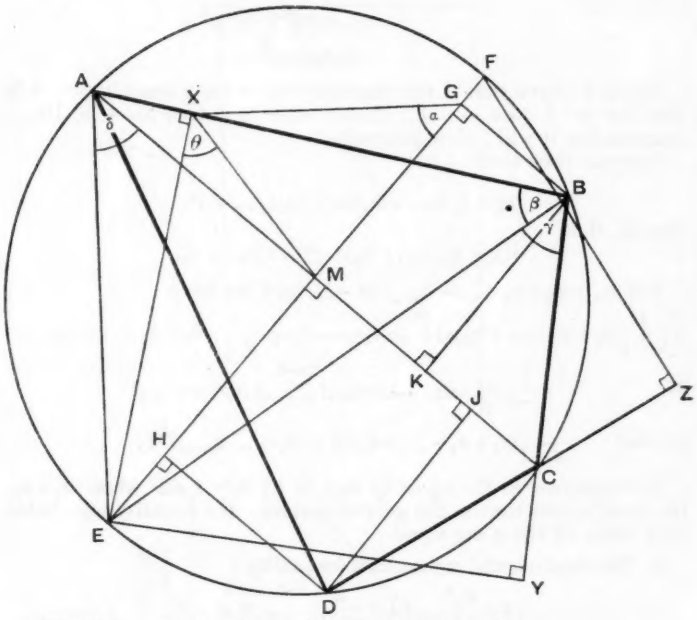
and $EM \cdot EF = EA^2$, since $\angle EAF = 90°$.

Thus $EM \cdot FG = AX^2$.

But $EG \cdot EF = EB^2$, since $\angle EBF = 90°$.

Whence, subtracting (3), we have

$$EG \cdot FM = BX^2.$$



Similarly, $FM \cdot EH = CZ^2$; $FH \cdot EM = DZ^2$.

Hence, from (2),

$$\text{area } ABCD = \sqrt{\{(DZ^2 - AX^2)(BX^2 - CZ^2)\}}$$
$$= \sqrt{\{(DZ + AX)(DZ - AX)(BX + CZ)(BX - CZ)\}}$$
$$= \sqrt{\{(s - AB)(s - BC)(s - CD)(s - DA)\}},$$

from (1), where $2s = AB + BC + CD + DA$.

The Manchester Grammar School. A. L. YOXALL.

1044. *Two inequalities.*

1. *The arithmetic mean of* n *positive magnitudes is not less than their geometric mean.*

The following proof, which I publish after a hesitation of some

years, is, I believe, essentially due to Cauchy, but this I have not been able to verify. At any rate it seems to be now forgotten.

Starting from the simple fact

$$\tfrac{1}{2}(a_1+a_2) \geqslant (a_1a_2)^{\frac{1}{2}}, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(i)}$$

we can show the theorem successively for 4, 8, 16, ... magnitudes, or for 2^r by induction. Thus

$$\begin{aligned}\tfrac{1}{4}(a_1+a_2+a_3+a_4) &\geqslant \tfrac{1}{2}[(a_1+a_2)(a_3+a_4)]^{\frac{1}{2}}\\ &=[\tfrac{1}{2}(a_1+a_2)]^{\frac{1}{2}}[\tfrac{1}{2}(a_3+a_4)]^{\frac{1}{2}}\\ &\geqslant (\sqrt{a_1a_2})^{\frac{1}{2}}(\sqrt{a_3a_4})^{\frac{1}{2}}\\ &=(a_1a_2a_3a_4)^{\frac{1}{4}}.\end{aligned}$$

We now prove that if the theorem is true for n magnitudes, it is true for $n-1$ (*not* $n+1$). Thence since it is true for 4, 8, 16, ... magnitudes, it will follow generally.

Suppose then that

$$(a_1+a_2+\ldots+a_n)/n \geqslant (a_1a_2\ldots a_n)^{\frac{1}{n}},$$

that is, that

$$[(a_1+a_2+\ldots+a_n)/n]^n \geqslant a_1a_2\ldots a_n.$$

Put $a_n=(a_1+a_2+\ldots+a_{n-1})/(n-1)$, and we have

$$\left[\frac{1}{n-1}(a_1+a_2+\ldots+a_{n-1})\right]^n \geqslant a_1a_2\ldots a_{n-1}\cdot\frac{1}{n-1}(a_1+a_2+\ldots+a_{n-1}),$$

or

$$\left[\frac{1}{n-1}(a_1+a_2+\ldots+a_{n-1})\right]^{n-1} \geqslant a_1a_2\ldots a_{n-1},$$

so that

$$\frac{1}{n-1}(a_1+a_2+\ldots+a_{n-1}) \geqslant (a_1a_2\ldots a_{n-1})^{\frac{1}{n-1}}.$$

Furthermore, as the equality sign in (i) only holds when $a_1=a_2$, the proof shows that in the general formula, the equality sign holds only when all the a are equal.

2. *The fundamental exponential inequality.*

$$\left(1+\frac{x}{n}\right)^n > \left(1+\frac{x}{n-1}\right)^{n-1}, \text{ if } x>0.$$

Take $n-1$ magnitudes equal to n^2+nx-n, and one magnitude $n(n-1)$, and apply paragraph 1. Thence

$$\frac{(n^2+nx-n)(n-1)+n(n-1)}{n} > [(n^2+nx-n)^{n-1}\,.\,n(n-1)]^{\frac{1}{n}},$$

or

$$[(n+x)(n-1)]^n > (n+x-1)^{n-1}\,.\,n^n\,.\,(n-1),$$

and so

$$\left(1+\frac{x}{n}\right)^n > \left(\frac{n+x-1}{n-1}\right)^{n-1} = \left(1+\frac{x}{n-1}\right)^{n-1}.$$

H. G. FORDER.

1045. *Length of Ellipse.*

An approximate formula for the length of the quadrant of an ellipse, however eccentric, may be obtained from the three-centred circular-arc construction published in *Gazettes* 194 and 197.

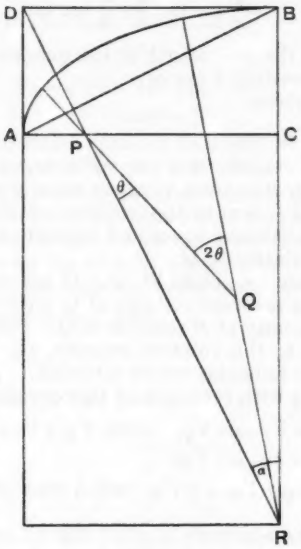


FIG. 1.

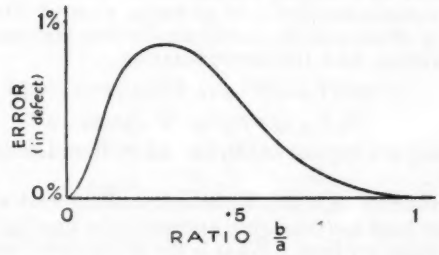


FIG. 2.

$ACBD$ is a rectangle ($CA = a$, $CB = b$), and DPR is drawn perpendicular to AB. Q is a point such that

$$QP = QR = \tfrac{1}{2}(RB - PA) = \frac{1}{2}\left(\frac{a^2}{b} - \frac{b^2}{a}\right).$$

The sum of the three arcs is then

$$\frac{b^2}{a}\left(\frac{\pi}{2}-\alpha-\theta\right)+\frac{1}{2}\left(\frac{a^2}{b}+\frac{b^2}{a}\right).2\theta+\frac{a^2}{b}(\alpha-\theta),$$

$$=\frac{b^2}{a}\left(\frac{\pi}{2}-\alpha\right)+\frac{a^2}{b}.\alpha$$

$$=\frac{b^2}{a}\arctan\frac{a}{b}+\frac{a^2}{b}\arctan\frac{b}{a}.$$

This is less than the true length of the quadrant of the ellipse by an amount not exceeding 1 per cent.

Felsted School, Essex. E. H. LOCKWOOD.

1046. *Relative Velocity.*

Definition: The velocity of a point P relative to a point Q is the velocity with which P appears to move when viewed from Q.

Notation: Let V_P denote the velocity of P, V_{-P} the reversed velocity of P (*i.e.* a velocity equal and opposite to that of P), V_{P-Q} the velocity of P relative to Q.

Consider two moving points P, Q. If we compound with the velocity of each the reversed velocity of Q, we bring Q to rest without altering the velocity of P relative to Q. The resultant velocity of P is thus equal to this relative velocity, *i.e.* $V_P+V_{-Q}=V_{P-Q}$, where the plus sign indicates vector addition.

Compounding V_Q with each side of this equation, we get

$$V_P=V_{P-Q}+V_Q, \quad \text{since } V_Q+V_{-Q}=0.$$

Similarly $V_Q=V_{Q-R}+V_R$.

Hence $V_P=V_{P-Q}+V_{Q-R}+V_R$, which may also be extended in the same way.

In every case the algebraic sum of the suffixes is the same on each side of the equation, and we can take a component from one side to the other by changing the sign of its suffix.

The two fundamental principles given on p. 39 of *The Report on the Teaching of Mechanics in Schools* are obvious and easily remembered when written with the above notation:

(*a*) $V_{A-C}=V_{A-B}+V_{B-C}$;

(*b*) $V_{A-B}=V_{A-X}+V_{-(B-X)}$.

The following are typical examples, taken from London Matriculation papers:

(1) To an observer on a ship which is travelling west with a speed of 20 miles per hour a second ship appears to be moving south with a speed of 15 miles per hour. What is the actual speed and direction in which the second ship is moving? In what direction and with what speed would the first ship appear to move to an observer on the second? (June, 1929.)

Let A, B stand for the ships.

V_A may be represented by $-\!\!\leftarrow\!\!-$ 20.

V_{B-A} ,, ,, ,, $\downarrow$15.

$V_B = V_{B-A} + V_A$.

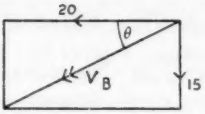


FIG. 1.

From the figure, $V_B = \sqrt{20^2 + 15^2} = 25$,

$$\tan\theta = \tfrac{3}{4}.$$

Hence B is moving with a speed of 25 m.p.h. at arc tan $\frac{3}{4}$ S. of W.
Also $V_{A-B} = V_{-(B-A)} = 15$ m.p.h. due north.

(2) A steamship is travelling north at the rate of 10 miles an hour and there is a north-east wind blowing at the rate of 20 miles an hour. In what direction will the smoke from the funnel appear to move to an observer on the ship ? (June, 1925.)

Let S, W stand for the ship and wind respectively.

V_S may be represented by $\uparrow 10$.

V_W ,, ,, ,, $\overset{20}{\swarrow}{}_{45°}$.

The required velocity $= V_{W-S} = V_W + V_{-S}$.

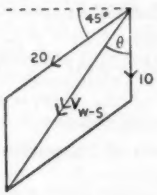


FIG. 2.

From the figure, resolving the velocities, we have

$$V_{W-S}\cos\theta = 10 + 20\cos 45°,$$
$$V_{W-S}\sin\theta = 20\sin 45°,$$

so
$$\tan\theta = \frac{20/\sqrt{2}}{10 + 20/\sqrt{2}} = \frac{2}{2+\sqrt{2}} = 2 - \sqrt{2}.$$

Thus the required direction is arc tan $(2-\sqrt{2})$ west of south.

D. A. YOUNG.

1047. *The Mean Value Theorem, and the Theorem of Proportional Parts.*

As bearing on Note 1002, in the *Gazette*, July 1931, the following way of presenting the above theorems may be of interest.

Theorem I. If $F(x)$, $G(x)$ are two differentiable functions which vanish when $x = a$, the ratio of their values when $x = b$ is equal

to the ratio of their derivatives when $x=\xi$, where ξ lies between a and b. That is,

$$F(b)/G(b)=F'(\xi)/G'(\xi). \quad \text{.....(1)}$$

For, consider the function $F(x)\,G(b)-F(b)\,G(x)$. It vanishes when $x=a$ and when $x=b$. Hence by Rolle's Theorem, its derivative vanishes when $x=\xi$, between a and b; and (1) follows in general.

Theorem II. If $F(x)$, $G(x)$ can both be differentiated twice, and if both vanish when $x=a$ and when $x=b$, the ratio of their values when $x=c$ is equal to the ratio of their second derivatives when $x=\xi$, where ξ lies between the greatest and least of a, b, c. That is

$$F(c)/G(c)=F''(\xi)/G''(\xi). \quad \text{.....(2)}$$

For, consider the function $F(x)G(c)-F(c)G(x)$. It vanishes when $x=a$, $x=b$, $x=c$. Hence, by an extension of Rolle's Theorem, its second derivative vanishes when $x=\xi$, between the greatest and least of a, b, c; and (2) follows in general.

Examples.

(i) To prove the Mean Value Theorem:

$$f(b)-f(a)=(b-a)f'(\xi). \quad \text{.....(3)}$$

In (1) put $F(x)=f(x)-f(a)$, $G(x)=x-a$, both of which vanish when $x=a$. Then (3) follows.

(ii) To prove Cauchy's formula:

$$\frac{f(b)-f(a)}{\phi(b)-\phi(a)}=\frac{f'(\xi)}{\phi'(\xi)}. \quad \text{.....(4)}$$

In (1) put $F(x)=f(x)-f(a)$, $G(x)=\phi(x)-\phi(a)$, both of which vanish when $x=a$. Then (4) follows.

(iii) To prove the formula of Proportional Parts:

$$f(c)-f(a)=\frac{c-a}{b-a}\{f(b)-f(a)\}+\tfrac{1}{2}(c-a)(c-b)f''(\xi). \quad \text{.....(5)}$$

In (2), put

$$F(x)=f(x)-f(a)-\frac{x-a}{b-a}\{f(b)-f(a)\},$$

$$G(x)=(x-a)(x-b),$$

both of which vanish when $x=a$, $x=b$. Then (5) follows.

(iv) To prove Taylor's Theorem:

We apply Theorem I, with a and b interchanged. Put

$$F(x)=f(b)-f(x)-(b-x)f'(x)\ \ldots-\frac{(b-x)^{n-1}}{(n-1)!}f^{(n-1)}(x),$$

$$G(x)=(b-x)^p, \quad (p>0).$$

Then $F(x)$, $G(x)$ both vanish when $x=b$. Hence, the ratio of their values when $x=a$ is equal to the ratio of their derivatives when $x=\xi$, between a and b. Hence we get Taylor's Theorem, with Lagrange's form of the "remainder" when $p=n$, Cauchy's form when $p=1$.

Generalisation. If $F(x)$, $G(x)$ can both be differentiated n times, and if both vanish when $x=a_1$, $x=a_2$, ... $x=a_n$, the ratio of their values when $x=b$ is equal to the ratio of their nth derivatives when $x=\xi$, that is,

$$F(b)/G(b)=F^{(n)}(\xi)/G^{(n)}(\xi), \quad \ldots\ldots\ldots\ldots\ldots\ldots(6)$$

where ξ lies between the greatest and least of $a_1, a_2, \ldots a_n, b$.

The proof follows from the corresponding generalisation of Rolle's theorem, and $a_1, a_2, \ldots a_n, b$ need not be distinct.

Example. To prove Lagrange's Interpolational Formula, with remainder :

Proof. In the usual way we find that the polynomial of the $(n-1)$th degree having the same values as $f(x)$ when $x=a_1$, $x=a_2$, ... $x=a_n$, is

$$\Sigma \frac{(x-a_2)(x-a_3)\ldots(x-a_n)}{(a_1-a_2)(a_1-a_3)\ldots(a_1-a_n)} f(a_1).$$

Then, in (6) put

$$F(x)=f(x)-\Sigma \frac{(x-a_2)(x-a_3)\ldots(x-a_n)}{(a_1-a_2)(a_1-a_3)\ldots(a_1-a_n)} f(a_1),$$

$$G(x)=(x-a_1)(x-a_2)\ldots(x-a_n),$$

both of which vanish when $x=a_1$, $x=a_2$, ... $x=a_n$, and we get

$$\frac{f(b)-\Sigma \frac{(b-a_2)(b-a_3)\ldots(b-a_n)}{(a_1-a_2)(a_1-a_3)\ldots(a_1-a_n)} f(a_1)}{(b-a_1)(b-a_2)\ldots(b-a_n)}=\frac{f^{(n)}(\xi)}{n!}$$

from which the required formula follows at once (cf. *e.g.* Whittaker and Robinson, *Calculus of Observations*, p. 34). F. BOWMAN.

1048. *How ought a logarithm to be defined ?*

The attached application to a simple number of a slightly modified form of Dr. Muirhead's method of calculation, which was explained by him under this heading in the December issue of the *Gazette*, is of interest because it shows how uneven the rate of *actual* approximation may be. If the attached table be symbolised in the form

$$2^a=b\times 10^c,$$

the proportional error in writing $2=10^{\frac{c}{a}}$ is $|1-b^{\frac{1}{a}}|$, which quantity remains stationary in the table below from $a=10$ to $a=100$.

$$\begin{aligned}
2^3 &= 0{\cdot}8 \times 10^1\\
2^5 &= 3{\cdot}2 \times 10^1\\
2^{10} &= 1{\cdot}024 \times 10^3\\
2^{20} &= 1{\cdot}04858 \times 10^6\\
2^{40} &= 1{\cdot}09951 \times 10^{12}\\
2^{80} &= 1{\cdot}20893 \times 10^{24}\\
2^{100} &= 1{\cdot}26766 \times 10^{30}\\
2^{103} &= 1{\cdot}0141 \times 10^{31}\\
2^{93} &= 0{\cdot}9903 \times 10^{28}.
\end{aligned}$$

Hence $\log_{10}2$ lies between 31/103 and 28/92, that is, between 0·30097 and 0·30107, and by proportional parts, $\log_{10}2 = 0{\cdot}30103$.

The complete calculation is as follows:

3·2	1·09951	0·9903
	9896	
9·6	990	1·024
64	55	1·0141
	1	
1·024		925
2048	1·20893	3
410	858401	
		0·30097
1·04858	1·20893	103
4194	4836	31
839	967	
52	60	1000
8	10	73
1·09951	1·26766	0·30107
		93
		28
		100
		7

and $0{\cdot}00010 \times \frac{141}{238} = 0{\cdot}00006$. W. J. H.

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874. "There is much meat in this first chapter for the biometrician, and also some highly amusing bits, like...the problem of how to cut a round tea cake, the consumption of which was to extend over three consecutive days, in such manner as to leave a minimum of exposed surface to dry. Galton solved the problem; Pearson is suspicious that Galton and his niece did not eat all the cake according to the solution, because on the third day roughly four-sevenths of the residue to be eaten was dry rind. A mere American's experience with English tea cake, extending at odd intervals over more years than he likes to think of, leads him to the view that the whole problem was supererogatory. No known English tea cake ever had sufficient moisture at any stage of its evolution or degustation to make the manner of its cutting of the slightest importance."—Raymond Pearl (reviewing Karl Pearson's *Life of Galton*) in *Science*, vol. 73, 1931, p. 239. [Per Dr. J. Wishart.]

875. Many of my contemporaries got their "elements" in the same old-fashioned way as I. They learnt the numbers as shapes and their "tables" of multiplication, and so forth, long before they realized what it was all about. Such propositions as "five and two are seven" or "twice five are ten" were learnt as dogmas. A single unsoundness in the set of dogmas thus acquired was enough to send all their summing wrong and give them a life-long dread of the uncertainties of calculation and the perplexities of mathematics. Their "sums" came out wrong, and there was no way of checking and discovering the faulty tendency. A little boy who had slipped into the heresy that $5+7=11$ or that $7\times8=48$ might have his life embittered and suffer punishments and impositions for years through the operation of that one undetected defect.—H. G. Wells, *The Work, Wealth and Happiness of Mankind*, p. 719 (1932). [Per Prof. E. H. Neville.]

876. The riddle he propounds to us has, like a quadratic equation, at least two answers.—From a dramatic criticism in *The Spectator*, 13th February, 1932. [Per Mr. A. V. Richardson.]

REVIEWS.

Almost Periodic Functions. By A. S. Besicovitch. Pp. xiii, 180. 12s. 6d. 1932. (Cambridge University Press)

The growth and development of real analysis has been bound up with the trigonometric series from the time of Fourier to the present day. The theory of trigonometric series has given off as by successive emanations the modern rigorous notion of function, the theory of sets of points, the Riemann and Lebesgue integrals, the Schmidt-Hilbert theory of the linear integral equation with symmetrical kernel, and Hilbert's space of a denumerable infinity of dimensions. The parent theory of all these disciplines has been that of series proceeding according to sines and cosines of integral multiples of an argument. Formally, the functions corresponding to such series have the property of periodicity.

It was, however, clear from the very beginning of the theory of trigonometric series that other functions than the purely periodic ones admitted some sort of a trigonometrical development. The Fourier integral is one method of assigning a trigonometrical development to an aperiodic function. Again, such a Dirichlet series as

$$\Sigma a_n n^{ix}$$

does not in general correspond to a periodic function. The oscillations of a linear resonator proceed according to sines and cosines of multiples of the time, but not in general of integral angles, and are accordingly not periodic. The same is true of the coordinates of many different sorts of non-linear dynamical systems, and aperiodic trigonometrical series have been found of great value in the study of the perturbations of astronomical orbits. It was through astronomy and dynamics that Bohl and Esclangon were led to seek a theory of a class of functions representable by trigonometric series and more general than the class of purely periodic functions.

However, the first really general and satisfactory theory was that of Harald Bohr. In 1924 he published in *Acta Mathematica* the first of a series of three fundamental papers entitled "Zur Theorie der fastperiodischen Funktionen". In this Bohr defines the property of being almost periodic. A function $f(x)$, real or complex, defined for all real arguments x, is said to possess a translation number τ_ϵ pertaining to the positive number ϵ, if for all values of x from $-\infty$ to ∞

$$|f(x+\tau_\epsilon)-f(x)| \leqslant \epsilon.$$

The continuous function $f(x)$ is then said to be almost periodic if, whenever ϵ is given, there exists a finite number l_ϵ, such that if y is any real number, the interval $(y, y+l_\epsilon)$ contains at least one translation number τ_ϵ pertaining to ϵ.

Bohr shows that if $f(x)$ is almost periodic,

$$A(\lambda)=\lim_{T\to\infty}\frac{1}{T}\int_0^T f(x)e^{i\lambda x}\,dx$$

exists for every real λ. He then shows that there is at most a denumerable set $\{\Lambda_n\}$ of values of λ for which $A(\lambda)\neq 0$. Let us write $A(\Lambda_n)=A_n$. Then Bohr's fundamental theorem asserts that

$$\lim_{T\to\infty}\frac{1}{T}\int_0^T |f(x)|^2\,dx=\sum_1^\infty |A_n|^2,$$

or in another form, that

$$\lim_{N\to\infty}\lim_{T\to\infty}\frac{1}{T}\int_0^T \Big|f(x)-\sum_1^N A_n e^{-i\Lambda_n x}\Big|^2\,dx=0.$$

This is an analogue of Parseval's theorem for Fourier series. Its simplicity gives a motive for the definition of almost periodic functions. Bohr's second paper makes this motive even clearer by showing that the class of almost

periodic functions is identical with the class of functions which may be uniformly approximated by polynomials of the form

$$\sum_{1}^{N} B_n e^{-i\Lambda_n x},$$

where N is a positive integer, $B_1, \ldots, B_n$ are arbitrary complex numbers, and $\Lambda_1, \ldots, \Lambda_n$ are arbitrary real numbers. Bohr's third paper is devoted to the extension of almost periodic functions to complex arguments.

Bohr's first two papers immediately evoked a considerable literature. Stepanoff extended the theory to cover certain classes of discontinuous functions. Wiener independently gave one of Stepanoff's generalizations, and brought the theory into relation with that of the Fourier integral, incidentally establishing Bohr's fundamental theorem by a new method. Bochner developed the analogue of the Fejér theorem concerning the summability of Fourier series. Besicovitch extended the Bohr-Parseval theorem to its widest possible scope.

Neither the first Bohr nor the first Wiener proof of the fundamental Bohr-Parseval theorem is easy reading. Bohr's proof is technically elementary, while Wiener's proof involves the Lebesgue integral, but has the advantage of tying together the Fourier integral, almost periodic functions, and the theory of spectra in optics. The first simple proof of the Bohr-Parseval theorem is due to Weyl, and depends on the methods of the theory of integral equations. Weyl also proves the approximation theorem of Bohr's second paper, and introduces a new extension of the notion of almost periodic functions. The expression

$$\lim_{T\to\infty} \frac{1}{2T} \int_{-T}^{T} f(x+y)\bar{f}(y)\,dy \qquad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

plays an essential part in Weyl's paper, and also in a second paper of Wiener, which establishes both of Bohr's fundamental theorems in a manner which is much simpler than that of Wiener's first paper, but which like it appeals to the theory of the Lebesgue integral. Wiener's proof of the approximation theorem is a modification of that of Weyl.

De la Vallée Poussin gives still another proof of the Bohr theorems. He also employs expression (1), but his proof is technically elementary, and is closely allied to the original method of Bohr. Granted the theories to which Weyl and Wiener appeal, their proofs are however at least as simple as that of de la Vallée Poussin, in the form in which he gives it. In the notes of a course of lectures given in Göttingen, Bohr adopted the de la Vallée Poussin method of proof, and it is the method which serves Mr. Besicovitch in his book on "Almost Periodic Functions".

Mr. Besicovitch has written the first treatise on this timely and important subject. His book covers everything that is central in the theory, leaving out certain work on special problems that are rather applications of the theory than intrinsic parts of the theory itself. It does not pretend to be encyclopaedic, and in many cases, only one of a group of independent proofs of a theorem is presented. With these restrictions, the book contains everything which it is necessary to know to carry out effective research in the field of the theory of almost periodic functions.

The first chapter is devoted to the theory of functions of a real variable which are almost periodic in the original Bohr sense. In 66 pages, the theory is developed with great thoroughness, detail and conciseness. It will be seen that the reviewer cannot be completely disinterested in his appraisal of the different proofs of the Bohr fundamental theorems, but he is forced to admit that Mr. Besicovitch has established his case in favour of the superior simplicity of his formulation of the de la Vallée Poussin proof. The first chapter also contains the arithmetical theory of almost periodic functions, as contrasted

with the analytic theory, together with a section on the Franklin-Bochner theory of almost periodic functions of several variables.

The second chapter deals with the successive extensions of the notion of almost periodic functions by Stepanoff, by Weyl, and by Besicovitch and Bohr in collaboration. This last extension covers the whole class of functions to which a trigonometrical polynomial can converge in the mean, and probably marks the last significant extension of almost periodic functions in the direction of relaxing the demands of continuity.

The third and last chapter covers almost periodic functions in the complex domain. It has many applications to the theory of Dirichlet series. It follows rather closely the ideas of Bohr's third paper, but at the end there is an extremely important discussion of the relation of the theory to more general types of almost periodic functions.

The book is a compact and a consistent whole. It may be used with profit by anyone oriented in the classical, pre-Lebesgue theory of Fourier series and functions of a real variable, and for the last chapter, in the barest elements of the theory of functions of a complex variable. Theorems belonging to a later stage of these theories are given *in extenso* before they are applied. The proofs are thoughtfully chosen, and there are no wide gaps which the reader must fill in for himself. On the other hand, the book is not easy reading, and demands the most careful attention at every line. This is inevitable in the present state of the subject, and perhaps will always be inevitable in any work involving such a mass of delicate, detailed analysis.

Typographically, the book is well up to the high standard of the Cambridge University Press. The reviewer would prefer a little more display of formulae on the page, particularly where formulae play so large a part as they do in the present work. He has not examined the proof-reading in detail, but should like to know why the name of F. Riesz is consistently spelled Riesc.

The book will be the standard work on the subject. There is at present a rapidly growing interest in harmonic analysis as applied to dynamics—see the recent work of Birkhoff, E. Hopf, Koofman, v. Neumann and Wintner—and this assures the theory of many useful applications. Accordingly, the reviewer believes that Mr. Besicovitch has written a work which will appeal to a large and permanent mathematical public.

NORBERT WIENER.

A Treatise on Photo-Elasticity. By E. G. COKER and L. N. G. FILON. Pp. xviii, 720. 50s. 1931. (Cambridge University Press)

It has been known since the time of Brewster that transparent materials such as glass or xylonite, which are optically isotropic so long as they are unstressed, lose that characteristic when strained either by external forces or by stresses developed in the course of manufacture, and acquire the property of double refraction. If polarized light is passed through a plate of the strained material, the part which is polarized in the direction of one principal stress (P) will be accelerated relatively to the part which is polarized in the direction of the other principal stress (Q); and if the plate is put between crossed Nicol prisms, then whereas no light passes through the combined system so long as the material is unstrained (since the second Nicol prism stops what has come through the first), this relative acceleration will result in colour effects which are determined by the stress-difference ($P - Q$). A means is thus presented for the exploration of any two-dimensional system of stress which may be beyond the power of mathematical theory to discover: for the thickness of the plate suffers a contraction which (if the material is elastically isotropic) is proportional to the stress-sum ($P + Q$); so, by exact measurements combined with photo-elastic observations, P and Q may be evaluated separately.

Modifications can be made in the technique. For example, using plane-polarized light it is possible to determine very accurately the *directions* of the

principal stresses at any point, and so to construct 'isoclinic' lines from which the lines of principal stress may be deduced. To the mathematician it is likely that this method will appeal more strongly—as being probably more exact—than the method of constructing isochromatic lines (of constant stress-difference) by a process of colour matching. But the colour effects have the more general appeal: they have made the photo-elastic method known to every engineer, as the most potent weapon at his disposal when confronted by complex problems of stress-distribution. For this weapon he knows that his gratitude is due primarily to two men, Professors Coker and Filon, who have made of the method their life-work. Now they have placed him still further in their debt, by collaborating to produce a treatise in which every aspect of that work is fully discussed.

Such a partnership, like that of Hindenburg and Ludendorff, tempts the student to make guesses as to how the work was shared. From their published papers one conjectures that Professor Filon's special interest has been in the underlying physics of photo-elastic phenomena, Professor Coker's in their application to practical problems; but in this book the dovetailing is so neatly done that guessing becomes a hazardous occupation. Chapter I gives an account of physical optics, based on the classical electromagnetic theory of light; Chapter II contains what is needed of the theory of elasticity for an understanding of photo-elastic applications; Chapter III is an historical summary of knowledge relating to the principles of photo-elasticity; in the remaining chapters (IV-VIII) the authors discuss various problems of engineering practice which they have had occasion to study by photo-elastic methods.

The book contains 720 pages, hundreds of line illustrations, sixteen plates in colour; the quality of its printing is what we have come to expect in the well-known "dark blue" series of the Cambridge University Press; it is written by men who have made its subject peculiarly their own. Of such a work, and more especially when the reviewer has found no leisure for adequate study, criticism is presumptuous, complaint ungrateful; all that is known of photo-elasticity is contained within its pages, and a reader can ask no more. But a scientific treatise must be judged in relation to the needs of those who will study it; and this reviewer confesses that he has found it difficult to decide what is the class of readers which the authors have had specially in mind. There is of course, first of all, the reader who is interested in everything that pertains to photo-elasticity, whether in theory or in practical application; who will study systematically both the fundamental theory of optics and of elasticity and the engineering problems treated in the concluding chapter. His needs are fully met, and he will find little to criticize beyond the weight of the volume which he has to hold: surely two volumes were indicated, both by the bulk and arrangement of the subject-matter?

But readers of this kind, one would suppose, are few in comparison with the many who are interested in photo-elasticity as a tool for research, and whose desire is to acquire such knowledge of its technique as will make them able to set about experiments, should occasion arise. Such readers will need some account of the undulatory theory of light, for they must have some mental picture of the phenomena of polarization; but it may be questioned whether, in order to acquire that picture, they should be asked to follow in detail the highly mathematical investigations which have given us a more or less complete theory of optics. Their interest is in phenomena—above all, in the birefringent property of strained glass or xylonite; if theory can account for those phenomena, well and good: if not, so much (from their standpoint) the worse for theory. And when they read that mathematical conclusions, even in regard to the phenomena of polarization, are sometimes (*cf. e.g.* § 1. 25) contradicted by experiment, will they not slacken in their resolve to understand photo-elasticity at such cost in mental effort?

Lastly there is the student to be considered, and the research worker in engineering science: for them, at a first reading, clear-cut statement is essential, devoid of scientific niceties. Thus we are led to a constructive suggestion, offered here in the conviction that the authors, by adopting it, could add greatly to the usefulness of their work, and without much cost in additional labour. What now is wanted, to supplement the treatise which lies before us, is a short manual for the laboratory, severely practical, which in itself would enable the student to set up his own apparatus, but at the same time, by copious references to the larger work, would stimulate him to closer study of the phenomena which he employs. Whether by the authors or by some other, this manual will be written, for it is sorely needed; if it is left to others, it may be badly done. So, though the authors may fairly claim to have given us already more than we had any right to expect, we can only hope that they, who can draw from such unequalled store of knowledge, may find the time to put us still further in their debt. R. V. S.

David Hilbert. Gesammelte Abhandlungen. I. Zahlentheorie. Pp. xiv, 539. Rm. 48. 1932. (Julius Springer, Berlin)

The first volume of Hilbert's collected works, which has just been published on his seventieth birthday, contains his contributions to the theory of numbers. As he is the doyen of the mathematicians of to-day, the reader will turn with considerable interest to the present volume to see what impression can be gathered from Hilbert's work. He will notice that the 550 odd pages contain eleven of Hilbert's papers or memoirs together with a short appreciation of his work by Hasse. The first is his beautifully simple version of the transcendence of e and π. The longest is his well-known report on the theory of algebraic numbers, and takes 300 pages. It was originally published in 1897 and has long been out of print, though fortunately a French translation has been available. Hilbert's report is a masterly production and immediately marks him out as one with an intimate and comprehensive knowledge of the whole subject, and with a great power of attractive and original presentation. His account of and contributions to the law of reciprocity are particularly valuable. Even at the present day, no one who wishes to master the subject of algebraic numbers would fail to read the report. Its preface also shows Hilbert to be one of the numerous distinguished mathematicians who have come under the spell of the theory of numbers.

The English reader will probably note at once the last paper published only so recently as 1909. It deals with the famous hypothetical theorem enunciated by Waring in 1782; that every positive integer n can be expressed as a sum of at most N rth powers of positive integers, where N depends only on r; for example, n can be expressed as a sum of at most nine positive cubes. We see here one of Hilbert's most remarkable characteristics, his able pioneer spirit, the seeking and solution of difficult, important and outstanding problems. Since then mathematics has been considerably enriched by other proofs due to Hardy and Littlewood and Winogradoff and the work of Landau in putting them in a simpler form and making them more easily available, so that Waring's theorem is known to a very large circle.

But unquestionably the most important and far-reaching of Hilbert's papers is the tenth one on the theory of the relative Abelian number-fields, republished in 1902 in the *Acta Mathematica*. Perhaps a crude indication of the subject might be useful. Suppose we are given an algebraic field $K(\theta)$, or say K, that is, the assemblage of the rational functions of θ with rational coefficients where θ is a root of an equation

$$a_0\theta^n + a_1\theta^{n-1} + \ldots + a_n = 0,$$

where the a's are rational integers. We may assume that this is the equation

of lowest degree of such equations satisfied by θ. Then any number ϕ of the field satisfies an equation

$$b_0\phi^n + b_1\phi^{n-1} + \ldots + b_n = 0,$$

where the b's are rational integers. In particular, ϕ is called an algebraic integer if b_0 is unity.

Suppose now that p and q are given integers in K. An obvious question is to find the conditions under which the congruence

$$x^2 \equiv p \pmod{q}$$

is solvable by an integer x of K. The problem is of course practically that of investigating the law of quadratic reciprocity in the field K. The solution depends on considering the relative quadratic field $K(\sqrt{p})$, that is, the assemblage of the rational functions of θ and $\sqrt{p}$ with rational coefficients. When the number of classes of ideals in K is odd (the concept of ideals is necessary because algebraic integers are not in general factorisable uniquely), the problem depends really upon principles due to Gauss and Kummer, and had already been considered in some of Hilbert's earlier papers. When, however, the class-number is even, entirely new principles are necessary. Hilbert was led to consider fields relatively Abelian with respect to K. By induction from some simple cases, he was able to initiate the beginning of a new theory and to state hypothetically a number of general theorems. These are, as he says, of wonderful simplicity and of crystalline beauty. Hilbert's work was developed and extended by Furtwängler, Takagi, Artin, Hasse and others. There now exists a tolerably complete and exceedingly beautiful structure reared upon the foundations laid by Hilbert. The subject is now generally referred to as the Theory of the Class Fields and forms the subject-matter of Hasse's Report. The subject is perhaps still unknown in Britain, though it is exceedingly popular in German universities, where it has attracted a large number of young enthusiasts.

Sufficient, however, has been said to show that Hilbert's work in the theory of numbers will give him a place with Gauss and Kummer. Mathematicians will indeed be grateful for the publication of this inspiring, useful and desirable volume.

L. J. MORDELL.

Foundations of the Theory of Algebraic Numbers. I. By H. HANCOCK. Published with the aid of the Charles Phelps Taft Memorial Fund. Pp. xxvii, 602. $8. 1931. (The Macmillan Company, New York)

I have been asked to review the present work, without awaiting the appearance of the second volume. The author was one of the last of Kronecker's pupils, and his book throughout is impregnated with ideas disseminated by Kronecker and Dedekind when the modern theory of algebraic numbers was being built up in the 'nineties. After preliminaries which are not always presented in strict logical order (*e.g.* the opening paragraph should come after the second chapter) there follow chapters on algebraic realms of rationality and algebraic integers, then four chapters (173 pages) on the modular systems of Kronecker and Dedekind. After this there is a long chapter (IX) giving an introduction to ideals, followed by chapters on the extended law of reciprocity, some cases of Fermat's last theorem, the correlation between quadratic forms and quadratic realms, the geometrical representation of ideals and cubic realms.

The germ of the modern theory of algebraic numbers lay in the discovery, early in the nineteenth century, of the fact that algebraic numbers are not in general uniquely expressible as products of prime factors. Thus

$$77 = 7 \cdot 11 = (8+\sqrt{-13})(8-\sqrt{-13}) = (5+2\sqrt{-13})(5-2\sqrt{-13}). \qquad (A)$$

Since the prime number 7 divides the product $(8+\sqrt{-13})(8-\sqrt{-13})$ it can be inferred that 7 has something in common with each factor of the pro-

duct. The entity common to 7, $8+\sqrt{-13}$ also divides $7x+(8+\sqrt{-13})y$ where x, y are any rational integers. This common entity is the aggregate of numbers of the type $7x+(8+\sqrt{-13})y$ and called an ideal, written in brief

$$\mathfrak{p}=(7, 8+\sqrt{-13}).$$

Similarly the factor common to 7, $8-\sqrt{-13}$ is the ideal

$$\mathfrak{p}'=(7, 8-\sqrt{-13}),$$

and the conditions that a, $b+c\sqrt{-13}$ should be a *basis* of an ideal are that a and b should be divisible by c, and b^2+13c^2 by ac. The product of two ideals $\mathfrak{b}=(a, b+c\sqrt{-13})$, $\mathfrak{b}'=(a', b'+c'\sqrt{-13})$ is the aggregate

$$aa'x+a(b'+c'\sqrt{-13})y+a'(b+c\sqrt{-13})z+(b+c\sqrt{-13})(b'+c'\sqrt{-13})t,$$

and, when a, a' are co-prime, it can be shown that this is identical with the aggregate $aa'x+(f+cc'\sqrt{-13})y$.

The norm of the ideal $\mathfrak{b}=(a, b+c\sqrt{-13})$ is $N(\mathfrak{b})=ac$, thus

$$N(\mathfrak{b})N(\mathfrak{b}')=N(\mathfrak{b}\mathfrak{b}'), \text{ a general rule.}$$

Putting $\mathfrak{q}=(11, 8+\sqrt{-13})$, $\mathfrak{q}'=(11, 8-\sqrt{-13})$, we now have

$$(7)=\mathfrak{p}\mathfrak{p}', \ (11)=\mathfrak{q}\mathfrak{q}', \ (8+\sqrt{-13})=\mathfrak{p}\mathfrak{q}, \ (5-2\sqrt{-13})=\mathfrak{p}\mathfrak{q}',$$

thus each of the factors of 77 in (A) is the product of two prime ideals.

It was shown by Dedekind about 1875 how to define ideals in any system of algebraic numbers in such a way that every ideal is expressible as a unique product of prime ideal factors, and that the norm of a product of prime ideals is the product of the norms of the factors. Two ideals $\mathfrak{a}$, $\mathfrak{b}$ are said to be equivalent if $\alpha\mathfrak{a}=\beta\mathfrak{b}$, where α, β are integers of the system, and equivalent ideals belong to the same class. Dedekind also proved that the number of classes of ideals in any realm is finite. Fundamental problems to be dealt with in any given realm are the calculation of the class-number, and the determination of the equivalence or otherwise of two given ideals.

Reverting to Prof. Hancock's book, the four chapters on moduls (V-VIII) are tedious, and it is doubtful whether a reader whose main purpose is to gain some knowledge of the theory of ideals can be expected to read them. No doubt Kronecker so expounded the subject in his lectures forty years ago, but ideals can be introduced more expeditiously now. The next long chapter (IX) gives an introduction to ideals in quadratic realms, and here as elsewhere Prof. Hancock often shows lack of conciseness. Methods of finding the primitive unit in a real realm, and the number of classes of ideals, are powerful enough to deal with $(\sqrt{m})$ when m is small, but it is questionable whether a reader, relying on the methods given in the book under notice, could find with reliability the class-number and primitive unit of $(\sqrt{228479})$ or the class-number of $(\sqrt{-100001})$. On the theoretical side a question which presents itself is why the basis of $(\sqrt{m})$ is taken to be 1, $\frac{1}{2}(1+\sqrt{m})$ when $m=4n+1$. Dedekind's theory, which is correct, was only obtained after many other theories of ideal numbers had been proved to be faulty, and it is natural for a reader to inquire why the basis of the integral elements in $(\sqrt{(4n+1)})$ should not be taken to be 1, $\sqrt{(4n+1)}$. Chap. XII gives an account of the correlation between quadratic forms and quadratic realms. Due to the determinant -11 there are three classes of Gaussian quadratic forms, as correctly noted in the tables in Werke II, the representative (reduced) forms being

$$x^2+11y^2, \ 3x^2+2xy+4y^2, \ 3x^2-2xy+4y^2.$$

On the other hand the class-number of $(\sqrt{-11})$ is 1, and the discrepancy is accounted for by the fact that the Dedekind theory of ideals is more correctly correlated with the quadratic forms $ax^2+bxy+cy^2$ where $D=b^2-4ac$. There is then only one class of positive quadratic forms for the determinant -11, a representative being $x^2+xy+3y^2$. On reading Chap. XII of the present work, in which there are no illustrative examples, the present writer

has been unable to extract the essential distinction just noticed. The last chapter gives some account of cubic realms as they were known at the time of the publication of Reid's dissertation thirty years ago. It is not without interest, provided the reader is prepared to assume with the author that this subject still stands at the stage where Reid then left it.

In the matter of the notation used, Prof. Hancock's choice of technical terms is not always a happy one. The correct rendering of the German Spur is spoor, and (p. 499) genera would be better than genuses. Prof. Burnside's phrase "integral basis" is more concise than "basis of all the integers", and "discriminant" is now generally accepted for the author's "basal invariant".

It is the traditional privilege of the reviewer, to show that he has read the book he notices and not merely the preface, to point out any mistakes in the book reviewed. On this occasion, to avoid the charge of drawing attention to printers' slips, I mention an error of principle, and another which comes twice. First, on p. 346 it is stated that realms in which there exist only principal ideals are those in which Euclid's algorithm for division is applicable. This is not always correct since, even among quadratic realms, there are some, such as $(\sqrt{-163})$, $(\sqrt{107})$ where all the ideals are principal but where Euclid's algorithm does not apply. Again, on p. 369 it is stated that the ideals $(3, 1+\sqrt{31})$, $(3, 1-\sqrt{31})$ are not equivalent, and on the following page the class-number of $(\sqrt{31})$ is stated to be 3. All the ideals in $(\sqrt{31})$ are principal, however; in fact $(3, 1+\sqrt{31})=(11+2\sqrt{31})$.

We hesitate to write more about the present work pending the appearance of the companion volume, where possibly some of the points now mentioned will be cleared up. The main criticism is that the first half of the book is too formidable as an introduction to the second. We feel that the reader of a 600-page book on algebraic numbers has a right to expect more of concise expression and less that is dispensable. On this account the book can hardly be recommended for the use of students. Retired naval and military officers, who frequently take up the theory of numbers, will find in it much to browse over.

W. E. H. B.

Partial Differential Equations of Mathematical Physics. By H. BATEMAN. Pp. xxii, 522. 42s. 1932. (Cambridge University Press)

Reviewers are wont to begin by extolling the virtues of the subject of the book on the table, which forms an easy opening gambit for the next move in the game; to bestow praise, loud or faint, on the author's competence, craftmanship and compass.

So extensive a domain as that of partial differential equations is, however, not easy to characterise, except by its diversity. Its claim to be the most beautiful in the realm of pure mathematics may find many supporters and as many dissentients; with greater certainty it can stand as the most suggestive branch of analysis.

The extent of the domain is such that a complete and systematic survey must needs proceed in stages, three at least, each with a separate aim and a special technique. The first is the demonstration of the existence of solutions and their separation into specific classes. The next is an endeavour to obtain the solutions, now known to exist, as explicitly as possible. Actually the direct integration of a partial differential equation is seldom, if ever, achieved; a solution of the requisite type or degree of generality is almost invariably evolved by synthesis from solutions of subsidiary equations of a known type—in particular, ordinary differential equations. Then follows the third stage, the problem of moulding the solution into a form adapted to whatever physical problem may be lurking in the background, by the imposition of appropriate boundary conditions.

The present work takes the matter up at this third stage, and although it does not specifically require, it is worthy of a knowledge of the earlier stages.

Unfortunately, there exists no satisfactory up-to-date English account of the pure theory of partial differential equations in a convenient and inexpensive form.

The author's object cannot better be stated than in his own words: "In this book the analysis has been developed chiefly with the aim of obtaining exact analytical expressions for the solutions of the boundary problems of mathematical physics". This object has most admirably been achieved. Admittedly, certain topics of the deepest interest have not received the consideration they deserve. Thus there is no detailed account of the methods of approximation to which so much attention has been directed of recent years, though a most valuable introduction gives an account of the basic principles of the more important of these methods, and indicates lines along which further investigation might proceed. But specialisation is necessary. Though mathematical works have tended to reach enormous, even repellent, size, the omnibus or outline-of-everything volume has not intruded its fungoid growth into the field of analysis.

There are twelve chapters in the main body of the book. Chapter I (114 pp.), on "Classical Equations", begins by illustrating the use of a Green's function in the simplest one-dimensional boundary problem, with passage to the limit when a condition is imposed at every point of the interval considered. The idea of the Green's function dominates the chapter, which contains sections on two-dimensional wave motion, conjugate functions, potentials, the classical equations for Euclidean space, and primary solutions.

The fundamental importance of the classical equations is acknowledged; nor can it be denied that their early investigators incidentally laid the foundations of modern analysis. Later developments, too, have never failed to suggest collateral lines of enquiry, in fields so diverse as groups and integral equations. But what is perhaps not so clearly realised is that whereas, in the earlier days, physical intuition proved to be a wise and usually a sure guide, in the present state of development it is far less to be relied upon when the problem is what to ignore and what to retain. Purely analytical methods have now cast out the old-fashioned art of instinctive manipulation.

Chapter II (89 pp.), entitled "Applications of the Integral Theorems of Green and Stokes", deals in the first place with Riemann's method (though there is no mention of Riemann's original writings), with Green's function in detail, and with the derivation of differential equations from a variational principle. Bateman remarks that because of the affinity between problems in differential equations and problems in the calculus of variations, it is often less laborious, in carrying out a change of variables, to perform the manipulations on the variational integral than to work with the differential equation.

The main topics in Chapter III (62 pp.), "Two-Dimensional Problems", are simple solutions and the methods by which they may be generalised (Fourier integrals); approximation to solutions by a method whereby the differential equation is replaced initially by a partial difference equation (a new application of an old principle); the potential function with assigned values on a circle, leading to Poisson's integral; and the analytic character of a regular logarithmic potential.

Chapter IV (63 pp.), on "Conformal Representation", is a profound monograph in itself. The main section headings are: the bilinear transformation; Riemann's problem; mapping of a unit circle on itself; the Green's function; mapping of polygons; and orthogonal polynomials connected with a given closed curve.

Chapters V to X deal exhaustively with the conventional types of boundary problem in three variables, by the use of Polar, Cylindrical, Ellipsoidal, Paraboloidal, or Toroidal coordinates. The book ends with short chapters on "Diffraction Problems" (XI) and "Non-Linear Equations" (XII).

The book is in no sense an introduction to the subject; it is essentially

a specialist's book. Much of it is difficult, but necessarily so; little fault can be found with the exposition, except that the arrangement is somewhat haphazard; the manipulation is superb. For the research worker this volume is indispensable. Between four and five hundred authors are quoted, chiefly authors of the last two generations. A remarkable feature is the number of references to technological journals not generally regarded as literature for the professional mathematician. The problems which from time to time enlighten the analysis are practical in the real sense, and not bags of tricks hidden among a jumble of pseudo-technical verbiage. Three examples, taken at random, of these problems are: earthquakes, drying of wood, equilibrium of a soap film.

Bateman's treatise is likely to remain for many years the standard text on partial differential equations of mathematical physics. It may seem ungracious to thank the author by asking for more. But this work reveals the gaps which exist in kindred topics, where adequate texts are lacking. We venture to suggest that no better author could be found for a much-needed treatise under a title which he himself proposes: "Integral Equations of Mathematical Physics". E. L. I.

The Constitution of Atomic Nuclei and Radioactivity. By G. GAMOW. Pp. viii, 114. 10s. 6d. 1931. (Oxford; at the Clarendon Press)

The recent considerable advance in our knowledge of the properties and structure of the atomic nucleus is due firstly to the brilliant development of ideas and experiments by Rutherford and others, and secondly to the fundamental step, taken independently by Gamow and by Gurney and Condon about the same time, of applying the concepts and methods of quantum mechanics—and particularly that formulation of quantum mechanics to which the term wave mechanics is applied—to nuclear phenomena, in the first instance to those of radioactivity and artificial disintegration. Since Rutherford's demonstration of the existence of the nucleus, this is perhaps the most important step that has been taken in correlating the observed properties of the nucleus and deducing from them something of its structure.

There are some ideas whose first recognition depends on a flash of insight amounting to a stroke of genius, though once stated they appear so obvious that one is surprised that they have not been recognised before; and of such ideas the general conception of the significance, according to wave mechanics, of the phenomenon of radioactive disintegration appears to be one.

One of the great differences between classical and quantum mechanics is that according to classical mechanics a region in which a particle has a high potential energy is an absolute barrier to such a particle whose total energy is less than the maximum potential energy, whereas according to quantum mechanics it is possible for a particle to penetrate through such a region, in which, classically speaking, its kinetic energy would be negative. Gamow saw that here, in principle, lay the explanation of radioactive disintegration; the nucleus can be regarded as a structure consisting of elementary particles lying in a small region surrounded by such a potential barrier, through which nevertheless its constituent parts have a certain small probability of penetrating.

The development of this idea soon had two important successes; firstly, it disposed of the paradox which had previously been holding up the development of a satisfactory theory of nuclear structure, namely, the paradox that the smallest distance from the centre of the nucleus at which some component parts (those giving the low-velocity α-particles) can be situated, according to the classical theory, is considerably larger than the distance at which the nucleus is found from collision experiments to act as a *point* charge in repelling incident α-particles; and, secondly, even with drastic simplifying assumptions about the force on an α-particle near the nucleus, it gave a good account of the variation of the decay constant of an α-ray disintegration with the energy

of the α-ray emitted, not only of the approximate regularity of this relation, but also of the departures from regularity.

A considerable part of this book is concerned with these applications of wave mechanics to spontaneous α-ray disintegration and to the related problem of artificial disintegration by incident α-particles, which are worked out in some analytical detail. But the nucleus is so small in size, and the energies necessary to affect it are so large, that it is a most difficult object to investigate; direct methods are not many, and in constructing a theory of nuclear structure we have to make the best use of any evidence, direct or indirect, which we can get, and be thankful for it. Without going into any details of experimental technique, Gamow brings together and discusses all the available evidence on the subject, of which the main other lines are the energy relations exhibited by the mass defects of the nuclei, the γ-ray spectra, the "fine-structure" of α-ray groups, and the nuclear β-rays.

Two main points emerge from the general discussion, namely, the striking success of the first application of wave mechanics to nuclear problems, and the extreme difficulty of further advances in a quantitative theory of nuclear structure. The general behaviour of the positively charged components of the nucleus is perhaps now understood in outline, although absence of knowledge of the details of structure of an α-particle, and of the forces between α-particles at distances of nuclear dimensions, makes it difficult to extend the quantitative treatment beyond the point reached by Gamow; but the *rôle* of the electrons in the structure of the nucleus, and the nuclear β-rays to which they give rise, raise difficulties which are quite fundamental and to which at present there is no answer. We are constantly reminded of these difficulties in reading the book, by finding paragraphs here and there, which are concerned particularly with the nuclear electrons, included in special symbols to mark their provisional and speculative character.

In such a difficult subject, controlled speculation may be one valuable means of advance, and can do little harm provided its nature is always recognised; not the least merit of this book is that the distinctions between experimentally established facts, deductions from them, and speculations not fully supported by experiment, are kept clear, and speculation is explicitly recognised as such. The book is a good and clear survey of the present position of a developing subject, and can be recommended to anyone requiring a statement and discussion of the evidence available at the time of writing. D. R. HARTREE.

(1) The Physical Principles of the Quantum Theory. By W. HEISENBERG. Pp. xii, 186. 8s. 6d. 1930. (University of Chicago Press: Cambridge University Press)

(2) An Introduction to Quantum Theory. By G. TEMPLE. Pp. 196. 12s. 6d. 1931. (Williams and Norgate)

(3) An Outline of Wave Mechanics. By N. F. MOTT. Pp. ix, 155. 8s. 6d. 1930. (Cambridge University Press)

(4) Band Spectra and Molecular Structure. By R. DE L. KRŌNIG. Pp. x, 164. 10s. 6d. 1930. (Cambridge University Press)

Now that the basis of the new Quantum Mechanics has been well established, and several mathematical techniques invented for dealing with problems of electronic motion, numerous books have appeared during the last two or three years dealing with various aspects of the subject from many different standpoints. Quantum mechanics has in fact reached the text-book stage, and present developments are mainly elaborations of the theory and further applications of it to physical and chemical problems.

Mathematically, quantum mechanics is a calculus for dealing with the motion of electric particles and systems of particles under various conditions and in various fields of force; and the mathematical theory has no part to

play either in the physical significance of the concepts or in the ultimate objective structure of the particles. While ignoring these questions, or at any rate leaving them undecided for the time being, rules have been developed to correlate the microscopic motions of the particles with observations of various phenomena. It should be noted that they are rules by which the results of observations may be predicted, and they take into account the reactions of the microscopic motions to the process of observation. They do not tell us anything about unobserved particles, but only how they behave when under observation. It is found that the idea of strict determinism as understood in classical physics must give way to the idea of probability, and the probable behaviour of microscopic systems under observation is described by the new theory. It is determined in this way how a large assembly of microscopic systems will behave without determining the behaviour of the individuals separately. It is not that strict causality does not exist, but that we do not sufficiently understand the microscopic motions to predict them definitely and individually.

Closely related to the probability ideas is the famous "Uncertainty Principle" first enunciated by Heisenberg. Roughly this states that we cannot at the same time know with absolute accuracy both the position and the momentum of a particle; the more accurately the one is measured, the greater error is there necessarily introduced into the measurement of the other. This is a consequence of a fundamental crudeness in our measuring apparatus, and it has been found impossible to devise (even ideally) an apparatus which eliminates this. The principle has been given great prominence by physicists on account of its apparent fundamentality. It receives much attention in Heisenberg's book.

Interpreting the essentially mathematical rules of prediction of quantum mechanics, it seems that a certain duality can be assigned to electrons (and also to light particles). They appear to have both a particle and a wave nature, the particle being more or less guided in its path by the wave. The probability of finding a particle situated at any particular point depends on the intensity of the wave at that point, and the extent of any wave group corresponds to the uncertainty of position of an individual particle.

In Heisenberg's book, which consists of lectures given at Chicago, the experimental basis and verification of the theory is given, without very much mathematical detail, particularly in relation to the wave nature of electrons, as shown by experiments on diffraction, etc. Much space is devoted to the Uncertainty Relations, which are clearly explained, and numerous experiments devised for measuring the position and velocity of an electron, all of which lead to the same fundamental uncertainty. There is also a chapter on the Statistical Interpretation of the Quantum Theory. It is assumed that the reader is familiar with the general ideas of quantum theory, and a mathematical appendix is given, which is a useful summary of the mathematical theory from the point of view principally of matrices. It seems a pity that this part of the book is so condensed, and the reader to whom the theory is new would be well advised to read the appendix first, or at any rate in parallel with the main part of the book. To the reader who is familiar only with the mathematical ideas, the book should be very useful in adding a touch of reality to an abstruse subject.

The *Introduction to the Quantum Theory*, by G. Temple, is a well-written book on the mathematical theory. The approach is from rather a novel, but interesting, standpoint. As an analogy to hydrodynamical ideas, an electric fluid is imagined, whose density is proportional to the square of the amplitude of the wave function, thus furnishing a mind-picture of the statistical distribution. The analogy proves very fruitful in developing the theory from Bernoulli's Equation, but it appears to be rather overstressed and its usefulness strained. There are good chapters on quantum algebra, matrix mechanics,

relativistic wave mechanics, and numerous simple problems are worked out illustrating the general theory. The book is very clearly written and is a useful general survey of the subject without reference to any particular aspect of it. It is essentially mathematical in outlook.

The *Outline of Wave Mechanics*, by N. F. Mott, on the other hand, has been written from the point of view of the physicist who wishes to understand something of modern mathematical theory without reading a mass of details. The " wave " nature of the quantum mechanics is stressed by way of analogy with well-known phenomena in vibrating macroscopic systems. In this way it is shown how certain results may be expected in the various problems treated without actually carrying out the detailed mathematical working. Thus an analogy is drawn between the electron in an ionised hydrogen molecule going over from one atom of the molecule to the other, and the interchange of energy between two pendulums suspended from a horizontal string, and useful results are deduced. It must be remembered that these analogies have no physical significance, but are only formal mathematical analogies. The explanations in many places are very condensed, and therefore obscure in meaning; otherwise the book is interesting. There are some irritating misprints and slips.

The book on *Band Spectra*, by R. de L. Krönig, is a specialised work dealing with one of the many applications of quantum mechanics. It is an elaboration of a series of lectures delivered by Dr. Krönig at Cambridge dealing very ably, and in some detail, with a complicated subject. It is probably of little interest to the general mathematical student, but very illuminating in showing the power and scope of the new mechanics. J. H.

Die Gruppentheoretische Methode in der Quantenmechanik. By B. L. VAN DER WAERDEN. Pp. viii, 157. Rm. 9 (geb. 9.90). 1932. Grundlehren der Mathematischen Wissenschaften in Einzeldarstellungen, XXXVI. (Springer, Berlin)

This slim volume is a very useful summary of the application of group theory to quantum mechanics. It does not, of course, compete with the great work of Weyl, nor is it as detailed as the corresponding work by Wigner. Nevertheless, it provides an elementary introduction to this important subject.

The quantum theory is introduced by means of Schroedinger's wave mechanics. Then there follows a chapter on the theory of groups and their representations by means of matrices. These results are applied in the next chapter to the rotation group and the Lorentz group, a brief sketch being given of the theory of spinor analysis which was initiated by the author himself.

The next chapter on the spinning electron is the most unsatisfactory in the whole book. Even the complicated notation of spinor analysis appears to be insufficient for the explicit construction of Dirac's wave equation, and the treatment of this subject is unnecessarily complicated.

The fifth chapter discusses the permutation group in relation to the theory of atomic structure, and the volume concludes with a brief account of molecular spectra. G. T.

Philosophischer Versuch über die Wahrscheinlichkeit. By P. S. DE LAPLACE. Ostwald's Klassiker der exakten Wissenschaften, N. 233. Pp. vii, 211. M.9.60. 1932. (Akademische Verlagsgesellschaft, Leipzig)

Most Englishmen will doubtless prefer to read Laplace's *Essai philosophique sur les probabilités* in the original French; there is a convenient edition in the series *Les maîtres de la pensée scientifique* (Gauthier-Villars, 1921), from which, in fact, the present German translation has been made by Dr. Heinrich Löwy. The German edition, however, has the advantage of notes by Dr. H. Pollaczek-

Geiringer, who not only gives a sketch of Laplace's life and mathematical work and valuable bibliographical information about the *Essai*, but also indicates the trend of the recent important work on probability by R. von Mises. The book is thus a welcome addition to Ostwald's Klassiker.

F. P. W.

The Combination of Observations. By DAVID BRUNT. Second edition. Pp. x, 239. 12s. 6d. 1931. (Cambridge)

The aim of this book, of which the first edition appeared in 1917, is to give an account of the method of Least Squares. The treatment, therefore, deals with the theory of errors of observations, and follows this up with a discussion of the problem of the adjustment of observations involving a number of unknown quantities. The methods date back to Gauss, and the subject has become sufficiently standardised in the course of time for the author to consider it unnecessary to make in this section any alterations in a new edition. The detailed treatment of the method of least squares is particularly valuable. The general title of the book was justified on its first appearance by the addition of four supplementary chapters which were not regarded by the author as more than introductions to the subjects discussed. These were: alternatives to the Normal Law of Errors, Correlation, Harmonic Analysis and the Periodogram. Now in the growth of the comparatively modern study of the theory of Statistics, brought about principally by the biometrical worker, use was made of many known results in the theory of error, but it was considered desirable to express these in a different notation. Thus the astronomer measures the accuracy of his results in terms of the constant of precision h, while the biometrician deals with the standard deviation σ. The fact that two notations have grown up side by side has been a little unfortunate for the development of the underlying theory, and it has certainly been perplexing to the student. For example, it is only now being realised that certain fundamental researches on the part of biometricians in the present century on the distribution of the standard deviation existed, in a different notation, in some work of Helmert published in 1876. Mr. Brunt chose to perpetuate this tradition in his book, and as a result, when he came to deal with biometrical developments, he was compelled to re-state some of his results in a different notation. It is very desirable that emphasis should be laid, by incorporation of both in a single text-book, on the identity of the theory in both cases, but this we feel might best be effected by a bold attempt at standardisation of the notation. In the new edition the chapter on alternatives to the normal law of error has been amplified, while multiple correlation has been introduced as an addition to the chapter on correlation. The years since 1917 have seen a very rapid development of the theory of statistics, and these alterations only serve to emphasise all the more the omission of other subjects of more immediate value to the practical research worker. Few workers have data so ample in character that they can investigate whether their universe departs significantly from normality or not. What concerns them more particularly are practical tests of significance which will enable them to distinguish between means, standard deviations and coefficients of correlation in different samples of data. The exact modern theory behind such tests finds no place in the book before us. To give one example, the only test mentioned for the significance of a correlation coefficient is a statement of its probable error, $0{\cdot}6745\,(1-r^2)/\sqrt{n}$. The author states that this applies where the frequency distribution (we are not sure whether he means that of r itself or of the universe) is approximately normal and n is large. He does not say that the r of the formula refers to the unknown correlation in the universe from which the samples are supposed drawn, and he does not emphasise the non-normality of the distribution of r in repeated samples, even when the universe itself is normal. It would be appropriate in a book whose early chapters give the mathematical theory of

the subject to state the exact mathematical results on which the methods of the later chapters are based; the reader could then see for himself the limitations of any particular formula. As a matter of fact the distribution of r is so far removed from normality when r is high that the probable error is of little use, yet the only example in which an observed correlation is tested is where $r=0{\cdot}87$ and $n=38$, which is certainly not large. Improved methods of discussing the significance of a correlation coefficient are given in detail in a book referred to by the author, but this book has evidently not influenced his own treatment.

The treatment of multiple correlation is welcome, but here again the essential identity of the methods with the fitting of data by least squares loses emphasis by the employment of a different notation. Not only so, but no tests are given for testing the significance of the regression coefficients when found, although the theory is implicit in the earlier determination of the probable errors of the unknowns in the chapters on least squares. The chief alterations are in the final chapters, for that on Harmonic Analysis has been almost entirely re-written, and the application of the method to the Periodogram has been revised. This is a subject that is peculiarly the author's own, and the treatment is full and leaves little to be desired.

The mathematician will find certain parts of the book where the proofs are a little sketchy. But he will be particularly concerned with the rather loose way in which the notation is sometimes used. Thus, if on p. 10, $\overline{x^2}$ is to be used for the mean of a number of squares, $\bar{x}$ is evidently an unsatisfactory representation of the square root of this quantity, particularly when $\bar{x}$ is used later, as on p. 21, for the mean of a number of x's. Again, it would seem desirable to distinguish in the notation the population parameter and the estimate that is derived for it from a sample. This applies on p. 56 in the determination of the mean square error of the mean square error, and a further criticism here is that it would have been desirable to indicate the point up to which the mathematical treatment was exact, and how beyond this the results were only approximate. Other points which occur to one to mention are: the appearance of an unexplained λ on p. 25, the double meaning of v on p. 62, *maximum* for *minimum* on p. 65 (carried over from the first edition), the omission of the words "divided by total frequency" in the definition of a *moment* on p. 135, the lack of generality in the statement of the Sheppard corrections on p. 140, the double meaning of x and y on pp. 155-6, a misprint in the first formula on p. 163, a and b for A and B on p. 205, and misprints on pp. 216 and 221. It is a little surprising to find no mention of tests of "goodness of fit" in the comparison of observed frequency distributions with the Gaussian and other laws of error. It is not enough to make a mere visual inspection of a curve drawn by hand, and it is not clear that the warning sounded by Karl Pearson so long ago as 1900 has yet been taken to heart. In one case the normal curve has been fitted, and although it is stated that the method of doing so is explained later, sufficient practical details are not given to enable the reader to fit even the normal curve to his data, so that the chapter on alternatives to the normal law of errors remains to a large extent academic.

J. WISHART.

What is "Probable Error"? By J. F. TOCHER. Pp. 63, with 24 diagrams. 1931. (Institute of Chemistry of Great Britain and Ireland)

This is a reprint of a lecture delivered before the Institute on 24th April, 1931, by one who is at once a chemist and a statistician. The lecture itself takes up some thirty pages, the remainder being devoted to a number of mathematical and tabular appendices, and to a very full collection of illustrative diagrams. Even so, it must have been heavy going for the audience, to whom much of the treatment would be unfamiliar, and only a lecturer with the gift of exposition that is undoubtedly possessed by Dr. Tocher would have

been successful in giving his hearers something useful to take home with them. The chemist who has to do much analytical work with samples on which he can in general make but few determinations is an eminently suitable subject for the exposition of the modern biometrical methods, and it is refreshing to see here a lucid account of these methods, which leaves most of the standard text-books high and dry, and gives, in the shape of practical statistical tests to be applied to the experimental data, the results of the best of recent theoretical researches. The title is a piece of journalism, for "probable error" as usually understood is soon left behind, and ingenious methods are sketched which enable one to dodge the older inverse probability line of argument in arguing from the sample of observations back to the characteristics of the population from which the sample is supposed drawn. The methods are, of course, perfectly general, and the pamphlet is commended to the attention of workers in other branches of science for methods which are not to be found in current text-books, but which are nevertheless of fundamental practical value. The author quotes Clerk Maxwell as saying that the calculus of probability is the true logic for this world, and the only mathematics for practical men. What will interest the mathematical reader is the development of the probability line of argument in the practical tests of significance. An illustration should make clear how very different is the line of approach from what it used to be. Consider the problem which is of most frequent practical occurrence. A sample of size n, where n is usually small in this kind of work, furnishes a mean value ($\bar{x}$) for the variate measured, and an estimate (s) of the standard deviation of the population from which the sample is supposed taken. The true mean and standard deviation are unknown, and the only assumption made is that the variate in question follows the normal or Gaussian law of distribution. The distribution of $t=(\bar{x}-a)\sqrt{n}/s$ in repeated samples of size n from such a population is known exactly, and for various assumed values of a the probability that t should be as great as or greater than this value can be calculated from the integral of the distribution, or more conveniently from a table of this integral, a condensed form of which appears as an appendix. It is thus possible to find two values a_1, a_2 ($a_1<\bar{x}<a_2$), such that the probability of a value $\bar{x}$ or greater having arisen from the population with true mean a_1 is only, say, 1 in 20, and the probability of a value $\bar{x}$ or less having arisen from a population with true mean a_2 and the same standard deviation as the first is also 1 in 20. Thus if either had been the true mean, the observed result would assume a certain limit (arbitrary, of course) of improbability. We may then say that it is rather unlikely that the true mean lies outside the limits a_1 and a_2. This method necessarily gives fairly wide limits within which the true value "probably" lies, particularly when the sample is a small one, but we have no right to expect more information about the population than the limited sample before us gives, and the value of the method lies in the small number of assumptions it is necessary to make. It is to be hoped that the example of the book before us will lead to these methods being adopted widely in practical work.

The book is very carefully compiled, and the author constantly uses numerical illustrations to enforce his points. The blemishes are few. By an obvious oversight two values in the table of t, for $t=7$ and 11, are wrong. The values of p for these entries should be ·9548 and ·9711, and the odds 21 to 1 and 34 to 1 respectively. J. WISHART.

Reason in Arithmetic. By E. A. GREENING LAMBORN. Pp. iv, 140. 3s. 6d. 1930. (The Clarendon Press, Oxford)

There must be few, if any, of us who have not been struck by the pernicious badness of everyone else's teaching. Mr. Lamborn is no exception to this universal law; and in fact the most successful part of his book is that which deals with the ineptitude of $n-1$ of the n inhabitants of this distressful

planet, not without some echo of the only recorded words of the late J. Horner (not the inventor of the approximate roots of equations, but the investigator of the contents of a Christmas pie).

In a vivacious and eminently readable first chapter on "Values in Arithmetic", the chief plum is a facsimile of a brass at Northleach dated "MCCCC47", the earliest English monument containing Arabic numerals, though curiously mixed with Roman.

Presently we are deep in quaint theories. We are to abolish the sign ÷, never write "24" but always "24·" (a plan not adopted in the numbering of the pages), and learn general scales of notation before using the decimal scale. Arithmetical problems are to be done by filling a line with figures and then crossing them out again, without regard to intelligibility or "checkability". Here is an actual quotation from page 49:

"Set down in reasoned form it is simply

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Does a "reasoned form" really mean one in which the dead figures outnumber the living by 20 to 8?

These gigantic massacres are apparently not incidents in the solution: they are the whole solution. On page 42 we read, "The written solution would appear simply as

$$\frac{11\frac{1}{2}.}{£5\frac{1}{2}d.} \times \frac{63}{12} \times 12 \times \frac{1}{2}0$$
$$= £396."$$

Yet after these wildernesses of decaying corpses we are told that "Arithmetical expression ought to have something of the charm of Tacitus and Bacon". Well, the charm, if any, of page 49 is much more like that of sausages and mashed—especially mashed.

Contrast with this the motto of a recent book of Arithmetical Puzzles: "Explain your work so that it may be easily understood".

In dealing with stocks Mr. Lamborn escapes for six pages from his crossing-out complex and provides his class with imitation bank-notes with which they float a Fried-fish-shop Company, with balance sheets, shareholders' meetings and a salaried managing director. "The result of this report is that Tomkins, whose salary at the local Motor Works exceeds his early notions of expenditure, approaches Smith to inquire if he will sell some of his shares. Smith, who is thinking of getting married, and wants money to buy a house, has been wondering how to get it." Such dramatisation is very good fun, and educationally effective when there is time to make a meal of it: if there is danger of confusion in hurried nibbles at it by the ordinary class and teacher, working against time with an overloaded programme, the fault is in the time-table rather than in the method.

It may be urged that the real abuses at which Mr. Lamborn tilts belong to the nineteenth rather than the twentieth century; and even the most hardened canceller will be sickened of butchered figures by his apotheosis of slaughter: yet so lively and polemic a book cannot fail to stimulate, not perhaps as the sun and sea-air stimulate, but like a mustard-plaster or the gadfly to which Socrates compared himself. Let us therefore accord to Mr. Lamborn's peculiarities "something of the charm of Plato and Fried Fish".

W. HOPE-JONES.

Geometry for Senior Schools. By W. JOHNSON. I. Pp. 72. II. Pp. 68. III. Pp. 64. 1s. each. 1932. (Oxford University Press)

These books form a useful three-year course for Senior Elementary Schools, presumably for children between the ages of eleven and fourteen. The course

is entirely "practical", and includes "plan and elevation" and something of isometric projection. Drawing-boards and T-squares and the other usual drawing instruments are used all through the set of exercises. Each book has revision exercises which may mitigate the effect of half-yearly promotions.

T. M. A. C.

Simplified Algebra. By H. McKay. Pupil's books: I, II, III. Pp. 64 each. 1s. 6d. each. Teacher's books: I. Pp. 108; II. Pp. 100; III. Pp. 96. 2s. 6d. each. 1932. (Oxford University Press)

The pupil's books contain a large number of graded exercises on which a good teacher might base a useful course of instruction—he will find the answers in the teacher's books, together with a number of oral exercises. The educational value of it all will depend on the way in which it is presented. There is still significance for us in the protests of De Morgan—nearly one hundred years ago—about the school-teaching of algebra as an art, and the possibilities of teaching it as a science.

T. M. A. C.

Elementary Trigonometry. By J. Prescott and H. V. Lowry. Pp. xi, 444. 5s. 1932. (Longmans)

In this book the authors have had in mind chiefly the practical side of the subject, although the theoretical side has by no means been neglected. That it is the work of experienced teachers is clear; the subject is treated fully, there are numerous worked examples, and scattered throughout the book are many useful hints to the reader on choice of method, possible traps, etc.

By extending the cosine rule for a triangle to two vectors and the included angle (of any magnitude), the authors lead up to a neat proof of the addition formula for angles of any size. An investigation of this formula by projection is also given.

A clear explanation is provided of the inverse functions and their principal values, and there are good chapters on the theory of the triangle, on applications to solid geometry and mensuration, and on the graphical solution of equations. There are plenty of exercises of all degrees of difficulty, with tables at the end of the book sufficient for their solution.

The last chapter begins by investigating approximations to the sine, cosine and tangent of a small angle. This piece of work is well done. The authors then obtain the infinite series for these functions by a method some steps of which, as they admit, are difficult to justify. This is disappointing, since any student who is able to understand the last part of this chapter should know enough of the calculus to enable him to obtain these series by simpler and more rigorous methods.

Although pains appear to have been taken to avoid introducing hazy conceptions of zero and infinity, the attempt has not been completely successful. For instance, on page 253 we read: "If the solution of an equation containing an unknown quantity x comes out in the form $x=0/0$, that is proof that the equation is an identity in x". One could wish the meaning had been expressed differently. Again, on page 40: "u/v is an infinitely large positive number when v is 0". But these lapses from a high level of excellence are rare.

The book is admirably adapted for use in class, and the authors are to be congratulated on producing a stimulating and interesting work.

C. J. A. T.

Elementary Calculus. By F. Bowman. Pp. ii, 286. 6s. 6d. 1931. (Longmans)

There is little to say of this book, which on the whole is a thoroughly efficient and very full introduction to the calculus. As usual, the technique of the subject is deferred, a large number of applications being deduced from very simple integrals. Much trouble has been taken with the numerous examples, to which there are answers. The book is very well got up and there is an index.

There is one very bad misprint in Chapter XII, where the phrase "logarithmic and *experimental* functions" persists for some pages. Furthermore, it must be decided some day whether the abbreviation $\fallingdotseq$ (used here) or $\simeq$ is to be used for "approximately equal to".

The treatment of logarithmic and exponential functions is perfunctory. I believe that Mr. C. V. Durell has got this right once and for all. His method of approach is to investigate $\int_1^x \frac{dt}{t}$, and to deduce all the usual properties therefrom. Such a method arises naturally in a book on the calculus.

It is not easy to see why the chapter on complex numbers, which comes last, was included. N. M. G.

First Ideas in the Calculus. By L. CROSLAND. Pp. x, 132. 2s. 6d. 1931. (Ginn)

In many schools the ideas of the calculus are taught only to the best boys as part of the syllabus of "more advanced" mathematics in the School Certificate examination. The author of this book suggests that these ideas might well be introduced to all boys two years before this examination, and that one period a week would be sufficient to cover the ground. The method is to continue the graphical work taught in the early stages of algebra, which was concerned with the drawing and reading of simple graphs, the noting of stationary values, and the illustration of different kinds of variation. It is suggested that now is the time to investigate the varying steepness of a graph and the meaning of the area under it.

The author's intention is carried out very successfully. At first the work is informal and descriptive, but the need for accuracy is soon emphasised. There are plenty of concrete and interesting examples. The book is nicely got up and there are answers to the examples.

But it will be difficult to find the time for this work. The reviewer is painfully aware of the ever-increasing pressure on the curriculum of a secondary school. It is to be hoped that some genius will arise who will write a comprehensive course of mathematics up to School Certificate standard. If this can be done so as to avoid overlapping there will be room for the substance of this book, which is a definite landmark in the teaching of mathematics.

N. M. G.

An Intermediate Geometry. By G. W. SPRIGGS and R. F. WARD. Vol. I. Pp. xii, 226. 5s. Vol. II. Pp. viii, 189. 4s. 6d. 1931. (Pitman)

In this book Volume I is concerned with the needs of non-specialists and Volume II with those of the specialist up to University Scholarship standard.

In Volume I the order of development is unusual and interesting. After three chapters on pure geometry, namely, harmonic section, properties of the circle and triangle, and vectors, there are three on the elementary analytical geometry of the straight line and circle. Then follows a chapter on the pure geometry of projection, and the subsequent chapters on the conic sections are based on the projection of the circle.

The authors consider that "in securing the appropriate intellectual attitude towards his environment the student shall be brought into contact effectively with geometry itself and not with a merely elegant algebraic travesty of it. . . . Emphasis is laid at all stages upon the elements of position, length and direction in whatsoever notation these be expressed, and the student should be brought to the clear realization of the fact that with these fundamentals in his possession all the rabbits are in the hat". As will subsequently appear, the reviewer does not agree about the rabbits, but does agree that by the close association of pure and analytical geometry, the latter is put at the service of the former, and the whole subject made clearer up to a point for the non-specialist student.

It should be noted that in this volume there are some exceedingly good diagrams, while both volumes are very attractively got up.

Volume II is concerned with focal properties, the general equation of the second degree, homogeneous coordinates and abridged notation, though none of these subjects is pursued very far. The last three chapters, namely, a prelude to modern geometry, the rise of modern geometry, and the parallel postulate, will be of absorbing interest to the student, and make this volume an important contribution to teaching practice.

But no mention is made of the philosophy of the circular points at infinity. There is no reference to the necessity for re-defining in analytical terms all the old geometrical ideas. General projection is not touched upon. The rabbits are *not* all in the hat. The Buck Rabbit is the gradual realisation that geometry *is* algebra, and that when the constants in the equations are real, then we can draw our figures if we wish.

Again, it will not do to emphasise the idea of vectors " with the express purpose of rendering definite ideas which are in constant demand ". This is but a step on the way to saying that " the nature of the universe is revealing itself under aspects which are essentially geometrical and highly abstract ". The fact of the matter is that in the theory of relativity a vector is defined by a set of differential equations, a geodesic by the condition that $\int ds$ is stationary, and the phrase " the universe is finite but unbounded " means nothing more and nothing less than that an extremely complex algebraical expression has a definite sign. Geometry in the old sense has entirely gone by the board, and analytical geometry has come into its own. It is a matter of the meaning of words. N. M. G.

Intermediate Calculus. By P. F. SMITH and W. R. LONGLEY. Pp. xiii, 457. 16s. 6d. 1931. (Ginn)

This book might well be called *Intermediate Pure Mathematics* rather than *Intermediate Calculus*. To support this statement and for ready reference in what follows, it is necessary to give a list of the chapter headings :

I. Collection of Formulas.
II. Review of Elementary Analytical Geometry.
III. Review of Differential Calculus : Algebraic Functions.
IV. Review of Integral Calculus : Algebraic Functions.
V. The Exponential and Logarithmic Functions.
VI. The Trigonometric Functions.
VII. Integration : Standard Forms.
VIII. Table of Integrals : Rational Fractions.
IX. Solution of Equations.
X. Empirical Equations.
XI. Polar Coordinates and Applications.
XII. Parametric Equations.
XIII. Curvature.
XIV. Series.
XV. Ordinary Differential Equations.
XVI. Solid Analytic Geometry.
XVII. Partial Differentiation.
XVIII. Double Integration.
XIX. Tables.

The first chapter includes the Greek alphabet with the names of the letters. This will no doubt cause American students to take an added interest in their fraternities.

In Chapter IV, p. 53, there is a mis-statement of fact. The fundamental theorem of the integral calculus is there stated to be, in effect, that

$$\lim_{n\to\infty} \sum_{i=1}^{n} f(x_i)\Delta x_i = \int_a^b f(x)\,dx.$$

This is not a theorem at all; it is merely a change of notation. There is nowhere in the book an attempt to *prove* that either of the above expressions is equal to $\phi(b)-\phi(a)$, where $\phi'(x)=f(x)$. This ought to have been done in Chapter VII, in which the technical processes of integration are well set out. Chapters V and VI are on conventional (and loosely reasoned) lines. In the reviewer's opinion, Chapters II, X and XI ought not to have been included, while in Chapter XIV most of the space is taken up with a discussion of elementary convergency. Maclaurin's and Taylor's series are treated very scrappily indeed, there being no reference whatever to the remainder after n terms. A further curious omission in the book is the lack of any reference to formulae for successive differentiation.

Chapter XVI is probably desirable in view of the inclusion of the next two chapters. In Chapter XVII there is the picturesque phrase "holding x fast". This and the word "formulas" are the only departures from the usual terminology. The book is well printed and bound: there are numerous examples, with answers, and an index. The table of integrals at the end is useful, but the numerical tables are quite unnecessary.

It is the reviewer's considered opinion that this book is not suitable for schools or colleges in England. It contains at once too much and too little.

N. M. G.

Mathematics of Finance. By D. H. Crenshaw, Z. M. Pirenian and T. M. Simpson. Pp. xiii, 383. 15s. (Published in U.S.A. 1930, and now in this country by Sir Isaac Pitman & Sons, Ltd.)

Though compiled specially for students taking a course in commerce and business administration in American polytechnics and universities, this is a book which may find a useful place in the counting-houses of many commercial establishments in this country.

It is complete in itself and sound within its limits. The first section of 140 pages gives an elementary explanation of the minimum amount of algebra necessary for the intelligent use of interest tables, and its mathematical range is indicated by its treatment of the binomial theorem. The properties of the coefficients are inferred from a study and comparison of the expansions of the first 5 powers of $(a+b)$, and the student is informed that in more advanced works on algebra it is proved that these properties are true for the expansion of $(a+b)^n$ when n is any positive integer. The second section of 121 pages deals carefully with the theory of compound interest to an extent sufficient for ordinary commercial purposes, and discusses questions connected with annuities, sinking funds, debenture bonds, and depreciation. A few pages on the mathematics of life assurance are added in a supplementary chapter. Both sections are plentifully supplied with illustrative examples and with questions.

The book ends with 104 pages of tables. Logarithms are given to 6 figures for the first 10,000 numbers and to 7 figures for numbers from 10,000 to 11,009, the 7-figure logarithms being specially useful for dealing with questions involving the rate of interest.

The usual compound interest functions are tabulated, namely, $(1+i)^n$, $(1+i)^{-n}$, the accumulation of annuities, ordinary annuity values and their reciprocals, all for 32 rates of interest and for 100 periods. Some useful little tables are also given for cases where interest is convertible 2, 4, 6 or 12 times a year. For use with the chapter on life assurance, rates of mortality and other functions are given according to the American table of mortality. This

is as good as any other table for the purposes of illustration merely, but in fairness the student should have been told that the table of mortality is an old one based upon the conditions of life in America in the middle of last century and that it is out of relation to the facts of the present day. The rate of mortality at age 10, the youngest age in the table, is ·00749. In the latest available life table for the population of England and Wales the corresponding rate for male lives of that age is ·00181, and the rate of ·00749 is not reached until after the age of 40. At the other end of the table it is misleading to suggest that 95 is the practical limit to life when modern tables are usually carried to the age of 105 or even further. D. C. F.

Contour Geometry. By ALEX. H. JAMESON. Pp. viii, 158. 7s. 6d. 1931. (Pitman)

There are numerous text-books on Descriptive Solid Geometry, but, in general, no application of the theories elucidated or the conclusions arrived at is demonstrated, the mathematical results being left to the individual to apply to his own particular requirements. In the text-book under review the author devotes the early portion of his work to the contour geometry of the straight line and plane and then considers Cones and Conicoids. The various theorems are concisely and clearly set out and the reader, having grasped these, is able to proceed to their practical application to problems of contours in earthwork and the calculation of volumes such as are met with in civil engineering practice. In these days of keen competitive prices and strictly economic payments the rule-of-thumb methods, often formerly in use for estimating quantities, are no longer tolerated, and exact methods must be used in order to obtain a high degree of accuracy. It is in this latter respect that the value of the book lies, as the engineer and surveyor has in it a guide to the precise use of contours and the necessary calculations for quantities, and it seems rather a pity that the author has given the somewhat brief title to his book of *Contour Geometry*, a title which perhaps does not suggest in itself the practical application of contour lines and their use for the estimation of earthwork quantities. A useful chapter is devoted to the methods of contouring, setting out earthwork slopes and the use of the mass haul curve. Examples of problems dealing with contours and volumes are solved and should be useful to students entering for the B.Sc. (Engineering) Degree of the University of London and the Associate Membership of the Institution of Civil Engineers. In short, the book should prove of considerable help and use both to the student as well as to the practical engineer and surveyor.

M. K. RICE-OXLEY.

PERSONAL NOTE.

DR. P. A. M. DIRAC has been appointed Lucasian Professor of Mathematics in Cambridge, following Sir Joseph Larmor's resignation.

Dr. Dirac took the B.Sc. Degree of the University of Bristol, first in Engineering and then in Mathematics. He entered St. John's College, Cambridge, as a research student in 1923 and proceeded to the degree of Ph.D. in 1926. He was elected a Fellow of the college in 1927 and a Fellow of the Royal Society in 1930. Dr. Dirac's work as a founder of modern quantum mechanics is, of course, world-famous.

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